IN THE UNITED STATES DISTRICT COURT
FOR THE DISTRICT OF MARYLAND

MARYLAND SHALL ISSUE, INC., et al.;   )
)
)
)
Plaintiffs,   )
)   **Case No.: 16-cv-3311-ELH**
v.   )
)
LAWRENCE HOGAN, et al.;   )
)
)
Defendants.   )
)

DECLARATION OF GARY KLECK

I, Gary Kleck, under penalty of perjury, declare and state as follows:

1.   I am more than 18 years of age and am competent to testify, upon personal knowledge, to the matters stated below.

2.   I am attaching a copy of my expert report in this matter as Exhibit A, the contents of which are, to the best of my knowledge and belief, true and accurate. I hereby adopt and incorporate that report as if set forth fully herein. My report enumerates my qualifications, experience, and cases in which I have testified.

3.   Defendants rely upon their disclosed expert, Professor Daniel Webster, to support their claim that a fingerprint requirement "acts as a deterrent to straw purchasers and those intending to purchase firearms solely for criminal purposes" and that the Handgun License "has been shown to be associated with a significant reduction in the number of handguns that have been diverted to criminals in Baltimore soon after retail purchase." Dkt. 59-1, at 23–24.

4.   To support this claim, Professor Webster relies upon a 2017 study he co-authored that purports to assess the Firearm Safety Act's ("FSA") impact on the supply of handguns diverted to criminal use in Baltimore. Dkt. 59-19, at ¶ 18 (citing Cassandra K. Crifasi et al., *The initial impact of Maryland's Firearm Safety Act of 2013 on the supply of crime guns in Baltimore*, 3(5) The Russel Sage Foundation Journal for the Social Sciences 128-140 (2017) (the "Baltimore Study")).

5.   The Baltimore Study concludes that the FSA caused a reduction in the supply of crime handguns in Baltimore, but this conclusion is not based on any actual (or reliable) data on the supply of crime handguns in Baltimore or anywhere else.

6.   The study utilized firearms trace data on guns recovered by police. For five reasons, these data cannot legitimately be used to assess the supply of crime guns: (1) the guns recovered by police are not representative of crime guns as a whole, or any subset of those guns, (2) samples of recovered crime guns overrepresent guns recovered by police soon after retail sale, (3) a short time-to-recovery is not an indicator that the recovered gun was "likely purchased with the intent of diverting that gun to a prohibited person," contrary to the authors' claim, and (4) the fact that a recovered gun was first sold at retail in a state different from the one in which it was recovered is not an indicator that it was trafficked or otherwise purchased with "the intent of diverting that gun to a prohibited person," contrary to the authors' claim, and (5) a gun recovered within one year of retail sale does not indicate that the gun was likely purchased with the intent of diverting the gun to a prohibited person, contrary to the authors' claim.

7.   Indeed, the ATF explicitly cautions potential users of their trace data that "[t]he firearms selected [for tracing] do not constitute a random sample and should not be considered representative of the larger universe of all firearms used by criminals, or any subset of that

universe." (2016 ATF trace report available at https://www.atf.gov/resource-center/firearms-trace-data-2016). Beyond not being a random sample, we know that police are more likely to request traces on guns that appear to be new (and that therefore are likely to be less than a year old) because traces on such recently sold guns are more likely to generate usable leads pointing to criminals who possessed the guns recently. Further, the ATF is more likely to be able to successfully trace recently sold guns since it is more likely that sales records still exist for newer guns. Exhibit B (Kleck and Wang 2009, pp. 1271-1272). Finally, samples of traced guns can also overstate the share of crime guns that had out-of-state origins. *Id*. Therefore, the trace data examined by the authors can tell us nothing whatsoever about the supply of crime handguns in Baltimore or Maryland, or whether the examined guns had been trafficked, rendering the study's conclusion useless.

8.   For these reasons, trace data cannot be used to draw conclusions regarding Baltimore crime guns or crime guns generally.

9.   Ignoring the meaningless trace data, a more direct measure indicates that the availability of guns to criminals actually increased after the FSA went into effect. The most direct measure of firearms availability among people willing to kill is the percent of homicides committed with guns ("PHG"). PHG reflects, within the subset of the population who are willing to kill, the share who had access to a gun. In the last three complete years before the FSA, 2010-2012, 69.3% (876 of 1264) of homicides were committed with firearms. In the first three complete years after the FSA, 2014-2016, 72.3% of homicides were committed with firearms, indicating that firearms availability among criminals willing to kill increased.

10.   The authors' conclusions are also based upon the results of a survey of Baltimore probationers and parolees regarding their perceptions of whether it became harder to get a gun

after the FSA went into effect. The authors concede that this convenience sample of criminals was not representative of criminals in general, in Baltimore or anywhere else. For this reason alone we can conclude that the authors' findings can tell us nothing about whether the FSA actually made it harder for Baltimore (or Maryland) criminals in general to acquire guns.

11.   This study is in any case irrelevant because the authors did not provide any evidence that the people surveyed had any qualifications to make this judgement. There is no evidence whatsoever that any of these individuals had actually tried to acquire a gun before or after the FSA went into effect (much less both before *and* after) – the necessary foundation in personal experience for a comparison on which they could base an assessment of whether it became harder for criminals to get guns after the FSA. The authors seemed to assume that just because these individuals were criminals, they must know how easy it is to get guns. This assumption is wholly unfounded and renders the study useless.

12.   Finally, 41% of the individuals answered "yes" to the question of "Have the new gun laws made it more difficult to get a gun?" and 54% answered "no." The 54% who answered "no" could have all believed that the law had made it easier to get a gun. If 41% of offenders thought the FSA made it harder to get a gun, but an even bigger 54% thought it made it easier, then overall the net effect of the FSA, as perceived by offenders, was to *reduce* the difficulty of getting a gun – precisely the opposite of the interpretation imposed on their findings by the authors. In any case, the authors' poorly worded questions rendered its results meaningless.

13.   Defendants also rely upon Professor Webster for the inference that a licensing requirement will cause a reduction in the homicide rate. Dkt. 59-1, at 24; Dkt. 59-19, at ¶¶ 14–16. To support this claim, Professor Webster relies upon two studies: (1) Kara E. Rudolph, et al., *Association Between Connecticut's Permit-to- Purchase Handgun Law and Homicides*, 105 Am.

J. of Public Health 8, e49 (Aug. 2015) (the "Connecticut Study"); and (2) Daniel Webster, et al.,

Effects of the Repeal of Missouri's Handgun Purchaser Licensing Law on Homicides, 91 J. of

Urban Health 2, 293 (2014) (the "Missouri Study").

14.   Professor Webster states that the Connecticut Study "found that the licensing

requirement to purchase a firearm was associated with a statistically significant reduction in

Connecticut's firearm homicide rates during the first decade that the law was in place, with no

similar reduction in non-firearm homicides." Dkt. 59-1, at 24; Dkt. 59-19, at ¶ 14. This study is

useless because it applied the synthetic control method improperly.

15.   To properly use the synthetic control method when studying a policy change, the

author must identify areas that, prior to the implementation of the new policy, had similar trends

in the outcome variable (the homicide rate in this case) as well as correlates of the outcome

variable. These areas are then combined into a single "synthetic control" unit whose trends in

homicide are used to simulate how homicide would have trended in the intervention area during

the post-intervention period had that policy not been implemented.

16.   One can easily determine how good the synthetic control is by how closely pre-

intervention homicide trends in the synthetic control correspond to the actual pre-intervention

homicide trends in the intervention area (in this case, Connecticut). Here, if pre-1995 homicide

trends in the synthetic control closely match Connecticut's homicide rate for the pre-1995 years,

then it is more likely that the synthetic control will provide an accurate representation of how

homicide in Connecticut would have trended without its PTP law. Conversely, if this

correspondence is poor, the method has no power to accurately estimate the impact of the PTP law

because gun homicide trends in the synthetic control area cannot tell the analyst how gun homicide

rates would have trended in Connecticut in the "counterfactual" situation where the PTP law was not passed.

17.   Here, the correspondence is poor. The correspondence is so poor that in many pre-1995 periods the synthetic control does not even trend in the same direction as Connecticut, much less match Connecticut's trends closely enough to serve the purposes of a synthetic control. For example, from 1984 to 1985, Connecticut's actual gun homicide rate declined, but the synthetic control's rate increased. From 1987 to 1988, Connecticut's firearms homicide rate increased sharply, but the synthetic control's rate declined. From 1989 to 1990, the synthetic control's firearm homicide rate likewise moved in the opposite direction of Connecticut's rate, as was true for the changes from 1990 to 1991, from 1991 to 1992, and from 1995 to 1996. There were 12 pre-1996 year-to-year changes in firearms homicide trends in the authors' dataset, and the synthetic control failed to even match the direction of Connecticut's changes in six of those 12 instances.

18.   Because the synthetic control is so poor in the Connecticut Study, its conclusions are meaningless and provide no support for Defendants.

19.   Professor Webster states that the Missouri Study "showed an abrupt increase in firearm-related homicides in Missouri after that state repealed its handgun licensing requirement in 2007." Dkt. 59-1, at 24; Dkt. 59-19, at ¶ 15. The study found a 25% increase in homicide in Missouri after the state repealed its permit-to-purchase law ("PTP"). Professor Webster also states that "the state experienced an increase in the percentage of crime guns recovered by police that had been originally sold by in-state retailers." Dkt. 59-1, at 24; Dkt. 59-19, at ¶ 16. Webster interprets this increase as an indication of changes in gun trafficking or illegal gun "diversion," but this is nothing more than guesswork – no such interpretation of trace data can be legitimately drawn.

20.   The study did not provide any scientifically reliable foundation for the conclusion that Missouri's PTP repeal caused a 25% increase in homicides. The authors used an inappropriate research design. Most gun law research assesses either (1) the effect of a gun control measure across a full set of jurisdictions that implemented that control; or (2) the impact of a gun control measure in a single specific area over a fairly long period of time. The Missouri Study authors, by contrast, used neither approach. Instead, they studied one change in a one type of gun control (PTP repeal) in one jurisdiction (MO) over one very brief period of time (2008–2010). Under this flawed design, *any* homicide-related factor that changed in that one jurisdiction in 2008-2010 might have caused its change in homicide rates. Although the authors controlled for poverty level, there are innumerous other factors which they did not control. All of these variables may have had effects on homicide that could have been confused with supposed effects of the PTP repreal, especially since the authors made virtually no attempt to control them. To my knowledge, the Missouri Study authors are the only researchers in the history of gun control research to use this curious research strategy.

21.   Additionally, the change in homicide that the authors attributed to the PTP repeal actually occurred in just one year, from 2007 to 2008. The Missouri homicide rate increased from 6.6 in 2007 to 8.3 in 2008. (Centers for Disease Control and Prevention, WONDER website available at http://wonder.cdc.gov/mortSQL.html. Accessed 4-2-18).   But after 2008 the rate declined to 7.3 in 2009 and then leveled off to 7.4 in 2010 and from 7.0 in 2010 to 6.1 in 2011 and 6.5 in 2012. *Id*. Thus, there was no lasting increase in total homicide after 2008, even though the PTP repeal remained in effect and presumably should have continued elevating the homicide rate, if it actually had the detrimental effects that Webster et al. attributed to it. Only the single homicide

increase from 2007 to 2008 supports the Missouri Study's conclusions. This broad conclusion rests upon a single data point.

22.   The study's assertion that Missouri experienced an increase in the percentage of crime guns recovered by police that had been originally sold by in-state retailers does not have the meaning that the authors attribute to it. There is no scientific foundation for interpreting this change as indicating a change in the frequency of illegal diversion of guns to criminals. Like the Baltimore Study, the Missouri study used the ATF's trace data, which cannot be used to draw conclusions about crime guns as a whole, much less gun trafficking or gun diversion. The National Research Council (2005) panel has concluded that "trace data cannot show whether a firearm has been illegally diverted from legitimate firearms commerce," and that "trace data analyses cannot describe the illegal pathways thorough which crime guns travel from legal commerce to its ultimate recovery by law enforcement." (National Research Council. 2005. *Firearms and Violence: A Critical Review*. Washington, D.C.: The National Academies Press. pp. 40, 80-81). Thus, the Missouri study's authors were wrong to believe that they could use trace data to measure illegal diversion of guns to criminals. For this reason, the statement that Missouri experienced an increase in the percentage of crime guns recovered by police that had been originally sold by in-state retailers cannot be used to draw any conclusions about changes in the rate of illegal gun diversion.
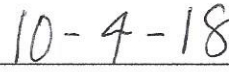
23.   Finally, it is logically impossible for the authors of any of the studies on which Webster relies to draw conclusions regarding the effects of requirements for fingerprinting, safety training, or any of the other specific elements of Maryland's FSA. Even if one set aside all the specific flaws previously noted, and the studies' methodologies were perfectly applied, the studies could at best only assess the overall, global effects of entire gun laws (which were composed of

multiple distinct provisions) being enacted or repealed. None of the studies can tell us anything whatsoever about the effects of any one component of the laws.

I declare and affirm under penalty of perjury that the foregoing is true and correct to the best of my knowledge, information, and belief.

Gary Kleck
Gary Kleck

10-4-18
Date

Expert Report in Maryland Shall Issue, Inc. et al. v. Hogan and Pallozzi

Gary Kleck

College of Criminology and Criminal Justice

Florida State University

Tallahassee, FL 32306

April 3, 2018

Exhibit A

My Qualifications

I am an emeritus Professor of Criminology and Criminal Justice at Florida State University. I received my doctorate in Sociology from the University of Illinois in 1979, where I received the University of Illinois Foundation Fellowship in Sociology. I was the David J. Bordua Professor of Criminology at Florida State University from 1978 to my retirement in 2016. My research has focused on the impact of firearms and gun control on violence, and I have been called "the dominant social scientist in the field of guns and crime" (Vizzard, 2000, p. 183).

I have published the most comprehensive reviews of evidence concerning guns and violence in the scholarly literature, which inform and serve as part of the basis of my opinions. I am the author of Point Blank: Guns and Violence in America, which won the 1993 Michael J. Hindelang Award of the American Society of Criminology, awarded to the book of the previous several years which "made the most outstanding contribution to criminology." More recently, I authored Targeting Guns (1997) and, with Don B. Kates, Jr., The Great American Gun Debate (1997) and Armed (2001).

I have also published scholarly research in all of the leading professional journals in my field. Specifically, my articles have been published in the American Sociological Review, American Journal of Sociology, Social Forces, Social Problems, Criminology, Journal of Criminal Law and Criminology, Law & Society Review, Journal of Research in Crime and Delinquency, Journal of Quantitative Criminology, Law & Contemporary Problems, Law and Human Behavior, Law & Policy Quarterly, Violence and Victims, Journal of the American Medical Association, and many other scholarly journals.

I have testified before Congress and state legislatures on gun control issues, worked as a consultant to the National Research Council, National Academy of Sciences Panel on the

Understanding and Prevention of Violence, was a member of the U.S. Sentencing Commission's Drugs-Violence Task Force, and, most recently, served as a member of the Institute of Medicine and National Research Council Committee on Priorities for a Public Health Research Agenda to Reduce the Threat of Firearm-Related Violence. I am a referee for over a dozen professional journals, and serve as a grants consultant to the National Science Foundation.

Prior to my retirement in 2016, I taught doctoral students how to do research and evaluate the quality of research evidence, and have taught graduate courses on research design and causal inference, statistical techniques, and survey research methodology. My current curriculum vitae is attached.

I am being compensated for my work at the rate of $400 per hour.

My Opinions

1. Professor Daniel Webster's study of the effect of Missouri repealing its permit-to-purchase (PTP) law (Webster et al. 2014) was fatally flawed, was not based on any scientifically reliable research design, and cannot be relied upon for purposes of assessing the impact of PTP laws on homicide.

2. The study of Connecticut's PTP law by Webster and others (Rudolph et al. 2015) employed a similarly unscientific research design, misapplied the synthetic control methodology, yielded findings that are as unreliable as those of the Missouri study, and therefore cannot be relied upon for purposes of assessing the impact of PTP laws on homicide.

3. The study by Crifasi et al. (2017) on the effect of Maryland's Firearm Safety Act of 2013 (FSA) on "the supply of crime handguns in Baltimore" has no actual data on the supply of crime handguns in Baltimore or anywhere else, relies on a long-discredited

misinterpretation of firearms trace data, and consequently provides no scientifically valid basis for judging whether the FSA had any impact on the supply of crime handguns.

4.   I know of no empirical evidence – either evidence cited by Professor Webster or any other - that Maryland's requirement for a "Handgun Qualification License" (HQL) will reduce any form of firearms violence beyond any effects produced by the background check that was already mandated under federal law before the HQL requirement was imposed. Even if one ignored the fatal flaws in the three aforementioned studies, their findings would be irrelevant to the current issue of whether the HQL is likely to produce any public benefit that would compensate for the burdens it places on Maryland's citizens, because none of these three studies, nor any others known to me, are capable of separating the effects of the elements in the HQL process from the effects of numerous other elements of PTP laws and of Maryland's FSA in particular. At best, studies of this type could only assess the overall global effect of PTP laws, not the effect of any one of their component elements.

The Basis for My Opinions

Opinion 1.

Regarding the study by Webster et al. of Missouri's (MO) repeal of its PTP law, the authors claimed that repeal caused a 25% increase in homicide. They did not provide any scientifically reliable foundation for this conclusion. The following comments explain why this is so.

Use of an Inappropriate Research Design

Webster et al.'s entire strategy for estimating an effect of MO's PTP repeal on homicide is both inappropriate for this purpose and contrary to customary scholarly practice in the field. Two broad categories of research design are used to evaluate the impact of gun control measures. First, studies of gun control impact attempt to estimate the effect of a given type of gun control (such as PTP laws) on violence rates *across the full set of jurisdictions that implemented that control*, comparing violence in these multiple areas with violence in multiple areas without such controls (see Kleck, Kovandzic, and Bellows (2016) for an example, and a review of similar prior studies). These studies use either a pure cross-sectional research design – studying many different areas at a single point in time – or a panel design that studies multiple areas in multiple time periods. With either design, the analyst assesses multiple implementations of a given type of gun control, in multiple jurisdictions.

Alternatively, other studies evaluate the impact of a specific gun control measure in a single specific area, using an interrupted time series design (see Britt, Kleck, and Bordua 1996 for a review and critique). Webster et al. uses neither of these approaches, adopting the unique strategy of applying a panel design to multiple states, observed over multiple years, but for the purpose of estimating the effect of a *single* change in a *single* type of gun control (a PTP law) in a *single* jurisdiction at a *single* point in time. To my knowledge, Webster and his colleagues are the only researchers in the history of gun control research that have ever adopted this curious research strategy.

There is good reason why previous researchers have not adopted this approach. Researchers studying the impact of a given type of gun control such as PTP laws in multiple

states are, in effect, estimating the average effect of these laws across the multiple states that have such laws. Thus, if twelve states have adopted PTP laws, the analyst has, in a sense, twelve opportunities to detect the effects of PTP laws. Simultaneously studying many implementations of a PTP law helps to rule out a large number of alternative explanations of lower homicide rates in places with PTP laws, because it is less likely that all or most of the twelve PTP states also share some other trait that actually causes their lower homicide rates. In contrast, if one studies only the implementation of a single PTP law (or its repeal) in a single jurisdiction, literally every homicide-related factor that changed in that one jurisdiction might have caused its change in homicide rates. The analyst then faces the hopeless task of trying to control for an immense number of likely confounding factors.

In the case of Webster et al.'s MO homicide study, the authors were, in effect, trying to estimate the effect of a single repeal of a single type of gun control (a PTP law) in a single jurisdiction (MO) in a single very brief time period (2008-2010). It must be stressed that they were *not* assessing the impact of PTP laws in general, though that would have been a quite reasonable course of action. They were instead applying a panel design to the assessment of a single change in gun law in a single place at a single time.

As a result, a change in virtually *any* homicide-related factor that occurred in MO around 2007-2010 could account for the state's homicide increase. The only specific confounding factors that Webster et al. can rule out as providing alternative explanations of MO's post-repeal homicide increase are the few that they explicitly controlled. Unfortunately, the only potentially confounding factor that Webster et al. explicitly controlled in their analysis of Uniform Crime Report (UCR) homicide rates was the poverty rate; all the rest of their control variables were not confounders, so controlling them did not help isolate the effect of the PTP repeal. And in their

analyses based on vital statistics data from CDC, Webster et al. likewise controlled for, at most, three variables that had a significant effect on homicide rates. Even these three, however, may not have been confounders because Webster et al. presented no evidence that they were correlated with the repeal of MO's PTP law.

The units of analysis in Webster et al.'s multivariate analysis of state homicide rates were the 50 states as they were observed in each of twelve years from 1999 to 2010 (or 1999-2012 in the UCR-based analyses). That is, each case for which they measured homicide rates and other variables was a "state-year" such as Missouri in 1998 or Florida in 2006. The single statistic on which they relied to draw their conclusions was the coefficient for a variable that was coded 1 for MO in 2008, MO in 2009 and MO in 2010, and coded 0 for all other state-years. Thus, the coefficient for this variable represents the average difference in homicide rates between (1) MO in the period 2008-2010 and (2) all other state-years, controlling for the other variables that Webster et al. included in their multivariate models.

The estimated value of this coefficient is entirely dependent on just how high homicide rates were in just *three* of the 600-700 state-years in the sample. Worse still, the change in homicide that Webster et al. attributed to the PTP repeal actually occurred in just one year, from 2007 to 2008. The MO total age-adjusted homicide rate increased from 6.6 in 2007 to 8.3 in 2008, but after 2008 the rate declined to 7.3 in 2009 and then leveled off to 7.4 in 2010 (CDC 2018). Further, UCR-based data indicate that the homicide rate declined from 7.0 in 2010 to 6.1 in 2011 and 6.5 in 2012. Thus, there was no lasting increase in total homicide after 2008, even though the PTP repeal remained in effect and presumably should have continued elevating the homicide rate, if it actually had the detrimental effects that Webster et al. attributed to it. Only the single homicide increase from 2007 to 2008 supports Webster et al.'s conclusions.

Thus, Webster et al.'s conclusion ultimately relies on the size of a *single* data point, the homicide rate in MO in 2008. If they were wrong about why homicide was higher in MO in 2008, their entire case for a homicide-elevating effect of the PTP repeal collapses. Thus, their conclusion rests on an extremely fragile foundation. As noted later in this comment, they had no reliable foundation for their claim that the repeal caused an increase in gun trafficking, illegal gun diversion, or gun possession among criminals, since they were not able to measure any of these things. Consequently, they had no basis for claiming that the repeal put more guns in criminal hands, regardless of the mechanism by which this might have occurred. Webster et al. ruled out only a single alternative explanation of the post-repeal homicide increase (changes in poverty) in their analyses of UCR homicide rates, so they had no sound basis for seizing on the PTP repeal as being responsible for the 2008 jump in MO homicide. As far as Webster et al. can demonstrate, it is just coincidence that this increase happened to follow the PTP repeal.

Why Study This Particular Change in Gun Law?

Over the past 50 years, homicide and other violent crime rates have increased about half of the time and decreased the other half (U.S. FBI 2017). Thus, if one randomly selected a change in gun control law, no matter how inconsequential it may have actually been, there is roughly a 50% chance that its implementation happened to coincide with a violence increase and a 50% chance that it coincided with a decrease. If one wanted to create an artificial appearance that a weakening of gun controls had caused an increase in violence, one would only need to identify any of the numerous instances of violence rates happening to increase just after some kind of gun law was weakened. Unscrupulous researchers might be tempted to cherry-pick from

among the thousands of gun law changes that have occurred in recent decades to selectively

study only those changes that suggested violence-reducing effects of gun control laws.

There are an extremely large number of changes to gun control laws to choose from. For

example, an analysis of legislation in Florida found that the legislature passed an average of 2.45

gun control bills per year over the period from 1973 to 1992 – a total of 49 changes in gun

control law in a single 20-year period in a single state (Etten 2002). Likewise, twenty-two states

enacted up to four different gun laws in the same year during the period 1977 to 2000 (Marvell

and Moody, 2006, Table 3). Across all 50 states, and the entire 1973-2014 period, the number of

changes in all types of gun control law would certainly number at least in the low thousands.

Given that violence was increasing in about half of those years, the number of instances in which

changes that weakened gun control coincided with violence increases would likewise number at

least in the hundreds. Of course, there would also be similarly large numbers of instances of

weakening gun control that coincided with *decreases* in violence. Regardless of analysts' biases,

they could easily find an ample number of instances to support their preferred conclusions.

Perhaps Webster and his colleagues felt that it was, for unspecified reasons, especially

important to study PTP laws. The question would still remain: why did the authors choose to

estimate the effect of *Missouri's* PTP law in particular? After all, there were at least nine states

with PTP systems in place c. 2000, and focusing on just one specific instance of this gun control

measure would guarantee that results would be more unstable and sensitive to controls for

confounding variables, compared to assessing the average effect of all available PTP systems.

In their conclusions the authors rather disingenuously wondered whether their findings could be

generalized to the PTP laws of other states (p. 300), without telling readers that they could easily

have resolved the issue by simply altering how they coded their main independent variable.

Instead of coding this binary variable 1 only for the absence of a PTP law in *MO*, they could have done the same thing for the absence of a PTP law in *all* of the state-years in their sample, thereby covering the entire U.S. The coefficient for this variable would then have represented the average treatment effect of the absence of a PTP law in all states, and there would have been no issue of generalizability. The authors do not provide any explanation of why MO's PTP law is any more important than other PTP laws, so it remains unclear why they focused on this particular state.

In sum, the approach used by these authors is useless for assessing the impact of changes in gun laws, but can easily be used to generate results that appear to support the researchers' policy preferences, whatever they might be, regardless of the actual effects of gun law changes.

Webster et al. Failed to Establish Any a Priori Plausibility for the Hypothesis

Webster et al. hint that repealing MO's PTP caused a 25% increase in firearm homicide. The authors use purely associational language in describing their results (e.g., the repeal was "associated with" a 25% increase in firearm homicide), but read in context, the implied meaning of a causal effect is unmistakable. Is there any *a priori* plausibility to a claim that a single seemingly trivial change in the details of MO gun law could, all by itself, cause a 25% increase in gun homicide?

Repealing the PTP law in MO did not eliminate background checks on firearms; all gun transfers by licensed gun dealers continued to be subject to a background check. Webster et al. argue that the key change produced by repeal of the PTP law was that it "eliminated mandatory background checks for handguns sold by unlicensed sellers." Whether this change was likely to be consequential, however, depends entirely on how often background checks on private transfers were performed before the PTP repeal, and how many blocked a gun transfer. If very

few or no such transfers were blocked before the repeal, there is no reason to expect that getting

rid of background checks on private transfers would have a measurable effect on criminal gun

possession, and thus on homicide. Webster et al. did not cite a single scrap of evidence that *any*

attempted private gun transfers were blocked under the old PTP system, and show no signs that it

even occurred to them that it was important for them to do so. Thus there is no evidentiary

foundation for believing the elimination of background checks on private transfers had any

measurable effect on the number of criminals who acquired handguns.

Guns "Diverted to Criminals"

Webster et al. claim that the repeal of the MO PTP law caused increased illegal diversion

of guns to criminals, which in turn increased firearm homicides. They do not define what they

mean by the term "illegal gun diversion," but a reasonable guess would be that it simply means

any illegal movement of guns into criminals' hands. Thus, it could encompass gun theft, guns

purchased from gun traffickers, guns illegally purchased from corrupt licensed dealers, guns

acquired through the use of straw purchasers, guns illegally purchased by convicted felons from

private parties, guns received as a gift by persons not lawfully entitled to possess guns, and a

host of other diverse ways that guns might illegally move into criminal hands. Webster et al.

appear to argue that repealing MO's PTP law caused gun possession to increase among

criminals, without being specific as to how or why this occurred. In any case, it needs to be

stressed that "illegal gun diversion," as Webster et al. use the term, does not necessarily refer to

gun trafficking in particular, and instead may refer to literally any illegal way guns might end up

in criminal hands. They provided no specific argument for which of these kinds of illegal

movements of guns increased after the repeal.   If, for example, the only kind of movement that

increased was gun theft, one might reasonably ask "why should the PTP repeal increase gun

thefts?" If the authors are correct that repealing PTP provisions made it easier for criminals to

buy guns, this should have *reduced* the need to steal guns, thereby reducing one major form of

"illegal gun diversion."

The indicator that Webster et al. used to measure "illegal gun diversion" was "the

percentage of guns that had unusually short intervals between the retail sale and the recovery by

police" (p. 294). Although it was unclear which set of "guns" the authors were referring to, it

turned out that the authors meant the set of guns recovered by police in connection with some

real or suspected criminal activity, submitted to the Bureau of Alcohol, Tobacco and Firearms

(ATF) for tracing, and successfully traced.   This measure, however, is not a valid measure of

either acquisition or possession of guns by criminals, or of gun trafficking, or gun "diversion,"

and has never passed any check of its validity as a measure of these concepts (Kleck and Wang

2009). In fact, the share of recovered "crime guns" with short times to recovery actually has a

weak *negative* association with a widely accepted indicator of trafficking, the share of recovered

guns that had an obliterated serial number (Kleck and Wang 2009, p. 1283). That is, Webster et

al.'s indicator actually tends to be *lower* where gun trafficking is higher. The authors did not cite

any validation studies that support use of this measure as an indicator of firearms diversion.

While it is true that many other careless analysts have also misinterpreted this indicator as a

measure of "firearm diversion or trafficking," it nevertheless cannot serve this purpose (Kleck

and Wang 2009).

These authors used trace data to draw conclusions about changes in the illegal diversion

of guns to criminals in general, an application that necessarily assumes that guns traced by ATF

can tell analysts something about crime guns in general. If findings pertaining to traced guns do

not apply to crime guns as a whole, analysts cannot legitimately draw conclusions about changes

in illegal gun diversion as a whole. Unfortunately, the set of guns traced by ATF is *not* a

representative sample of crime guns, or even of crime guns recovered by police, or any subset of

these populations of guns. There can be no honest doubt about this point among users of ATF

trace data, since ATF states a quite explicit disclaimer on this point: "Firearms selected for

tracing … do not constitute a random sample and should not be considered representative of the

larger universe of all firearms used by criminals, or any subset of that universe" (U.S. ATF

2013). This disclaimer only confirms the same conclusions that had been previously drawn by

the Congressional Research Service (1992) and the National Research Council (2005, p. 40).

More specifically, the National Research Council (2005) panel also concluded that "trace

data cannot show whether a firearm has been illegally diverted from legitimate firearms

commerce" (p. 40) and that "trace data analyses cannot describe the illegal pathways thorough

which crime guns travel from legal commerce to its ultimate recovery by law enforcement" (pp.

80-81). Thus, Webster et al. were wrong to believe that they could use trace data to measure

illegal diversion of guns to criminals. These facts about the trace data have been well-known for

decades (Congressional Research Service 1992; Kleck 1999), suggesting that Webster et al. were

either remarkably ignorant of the basic facts about the gun trace data on which they relied, or

they knowingly misled their readers as to what could be inferred from changes in time-to-

recovery among ATF-traced guns.

Thus, Webster et al.'s analyses of traced gun data can tell us nothing about trends in gun

trafficking or illegal gun diversion. They misused trace data for a purpose that the data cannot

legitimately serve. One cannot legitimately use trace data to infer anything about the guns used

to commit crimes, including the interval from their first retail sale to their recovery by police

(better known as "time-to-crime" (TTC) or "time-to-recovery" (TTR), and this time interval is

not an indicator of gun trafficking or any other form of "illegal diversion" of guns. Likewise, one cannot use trace data to track trends in gun trafficking or "illegal gun diversion."

Webster et al. concluded that repeal of MO's PTP law caused an increase in homicide. There is a disconnect between this conclusion about homicide and the authors' evidence concerning recovered guns. Even if short time-to-crime *were* a valid indicator of illegal gun diversion, and the guns chosen for tracing by police *were* a random sample of "crime guns" in general, analysis of general samples of traced guns still could not tell us anything about the guns used *in homicides*, since only a tiny fraction of traced firearms are recovered in connection with homicides. For example, among the 4,341 guns submitted for tracing in Missouri in 2011, only 134, or 3.1 percent were linked with homicide (ATF 2013). Indeed, few of these guns had been used to commit *any* violent crime – only 534, or 12 percent were linked with homicide, aggravated assault, or robbery. Instead, traced guns were most commonly linked with violations of gun control laws, such as unlawful "Possession of Weapon." In principle, Webster et al. might have used trace data to measure the average time-to-crime among MO guns recovered in connection with homicides, since those data are available, but chose not to do so. They therefore had no basis for believing that the guns used to commit homicides were moving more quickly into criminal hands after MO repealed its PTP law.

Webster et al.'s Comparisons of Homicide Trends in MO with Trends in Other States

Webster et al. reported simple comparisons of trends in age-adjusted firearm homicide rates in MO over the period 1999-2010 with trends in other states and with the U.S. as a whole (Figure 1 and Table 1). This is an unusually short period of time to analyze in this type of panel design, and is prone to extremely unstable results with respect to exactly what set of years one happens to analyze. Certainly data availability cannot explain the authors' decision to study so

few years, since state firearms homicide data are available from at least as far back as 1968. The

authors offer a different, bizarre explanation for why they used such an unusually short time

series, arguing that 1999-2012 was a period of stable homicide rates, which they speculate means

that an analysis of homicide rates will be less subject to omitted variable bias. There is no

justification of this type for short time series in the statistical literature, and the single source

they cite in support (see their source 16) does not in fact support the use of such a short time

series. Studying time periods in which there is little variation in homicide rates makes it *harder*

to determine what causes changes in those rates. In any case, a longer time period would have

provided a more stable set of estimates of the repeal's effects.

The results of longitudinal analyses of small numbers of time points can be radically

manipulated simply by analyzing arbitrarily selected subsets of the total set of time points for

which data are available (see Britt, Kleck, and Bordua 1996 for a direct demonstration of radical

changes in estimates of the impact of a gun law change when slightly different sets of years were

analyzed). If we repeat the authors' simple before-and-after comparisons of firearm homicide

rates, but use different sets of years to compare, we arrive at results quite different from theirs.

One could argue that 2007 should not have been treated as a pre-repeal year since part of the year

(after 8-28-07) was after the repeal was in effect. Using the last two complete years before 2007

as the pre-repeal baseline, the average firearm homicide rate was 5.2. And one could argue that

the lasting effects of the repeal would be better observed in a later set of post-repeal years, such

as 2011-2013, after the repeal had a longer period of time to show its full effects. The average

firearm homicide rate in those years was also 5.2, indicating that there was no lasting increase in

firearm homicide after the PTP law was repealed (CDC 2018) – exactly the reverse of the

conclusion drawn by Webster and his co-authors.

Webster et al. also compared homicide trends in MO with those in specific other states (their Table 1), and claimed that MO was unique in experiencing big gun homicide increases in this period (p. 299). In fact, comparisons of MO with other Midwestern states directly contradict their interpretation of the drops in firearm homicide in MO. MO did indeed experience a 36% increase in the firearms homicide rate from 2007 to 2008, but CDC mortality data show that Iowa's firearm homicide rate increased by a far larger 88%, from 0.60 in 2007 to 1.13 in 2008 (CDC 2018), even though Iowa did not repeal its PTP law, which was on the books in both 2007 and 2009 (see ATF 2005, p. 183 and ATF 2010, p. 202). Likewise, Nebraska, which neither repealed a PTP system nor weakened its gun laws in any other way, experienced a 59% percent increase in firearm homicide rates from 2006 to 2008 (CDC 2018).   In sum, both Iowa and Nebraska experienced jumps in their firearms homicide rates during this same period in which gun homicide increased in MO, and their increases were *bigger* than that of MO, suggesting that something other than weakening of gun laws was causing increases in firearm homicide rates in Midwest states in this period. Webster et al. were flat wrong to claim that "Missouri's sharp increase in firearm homicides was unique within the region" (p. 299). The only reason the authors were able to make it seem that big gun homicide increases were unique to MO was by employing a highly selective reporting of the state firearm homicide trend data available to them.

If the PTP provisions had been keeping down firearm homicide before its repeal, its continuing absence throughout the post-2007 period should have continued to contribute to higher gun homicide rates for years well after 2007. The authors' use of multi-year averages in Table 1 concealed the extreme instability of single-year state homicide rates, the fact that all of MO's homicide decline occurred in a single year, and the fact that other states experienced even larger one-year increases during this period. In fact, all of the post-repeal increase in MO

homicide occurred in just a single year, from 2007 (4.6 firearms homicides per 100,000) to 2008 (6.2). After 2008, MO experienced no further homicide increases, contrary to an interpretation that the PTP repeal was what was responsible for the post-2007 increase in homicide.

The point is not that we can tell anything useful about the reasons for homicide changes in MO (or Iowa or Nebraska) from these kinds of simplistic comparisons. Rather, the Iowa and Nebraska data demonstrate that single states can easily experience year-to-year homicide increases just as large as that observed in MO without it being due to the repeal of a PTP law or any other gun control measure, that it could happen at the roughly the same time as it happened in MO, and could happen in the same region of the country. Thus, these simple comparisons do definitively establish the simple point that MO's homicide increase could easily be entirely due to other factors, like those operating in neighboring Iowa or Nebraska, besides the repeal of a PTP law.

Webster and his colleagues also insist that there is something significant about the fact that this large homicide increase occurred specifically in the *firearm* homicide category. They appear to be unaware that, when homicide in general is increasing, *regardless of the reasons*, gun homicide always shows proportionally larger increases than nongun homicide. Even when gun law is unchanged and gun ownership levels are stable, one will still find that changes (upward or downward) are proportionally larger in the gun homicide category than in the nongun homicide category (Britt, Kleck, and Bordua 1996). The fact that gun homicide increased more than nongun homicide after the repeal of MO's PTP therefore tells us nothing about the likely causes of MO's short-term bump in homicide.

The Authors' Multivariate Homicide Analysis

The Near-complete Failure to Control for Actual Confounders

Webster et al. appear to address the possibility that other factors besides the PTP repeal were responsible for the MO homicide increase by controlling for other factors in their multivariate analysis of homicide rates in 43-51 states (including the District of Columbia), over the period 1999 through 2010 (or 2012 in some analyses). They controlled for changes in some other factors that might have affected changes in homicide rates, and still found a significant association between MO's PTP law and homicide rates.

Whether these analyses improved their ability to estimate the effect of the PTP repeal, however, depends entirely on the degree to which Webster et al. controlled specifically for *confounding variables*. A confounding variable is a variable that has *both* of two properties: (1) it has a causal effect of its own on the dependent variable (in Webster et al.'s research, the state homicide rate), *and* (2) is associated with the principle independent variable of interest (the existence or absence of the MO PTP law). If a variable lacks the first property it is not a confounder because it does not affect the homicide rate, i.e., it is an "irrelevant variable." If it lacks the second property, it does not matter whether the variable is statistically controlled, since estimates of the impact of the PTP law will be the same regardless of its inclusion in statistical models of homicide rates. Its inclusion is simply inconsequential.

Controlling for a confounding variable serves to rule out an alternative explanation of why homicide changed in MO after the repeal. Thus, if one controls for confounding factor X, one rules out the possibility that changes in X caused the homicide increases rather than the PTP repeal. The more genuine confounding variables that one controls, the more confidence one can have in the resulting estimate of the effect of the key independent variable.

Unfortunately, we can tell from Webster et al.'s own statistical results that they controlled for virtually no genuine confounders, and thus did virtually nothing to rule out any

specific variables as being responsible for the MO homicide increase. They report that only

three of their control variables were significantly related to homicide rates (p. 298), and thus

might be confounders.   All the rest of Webster et al.'s control variables lacked the first

necessary property of a genuine confounder, that they have a causal effect on the homicide rate.

Further, even the findings regarding two of Webster et al.'s three significant control

variables were perverse and contrary to theory and prior research.   The significant coefficient for

poverty was *negative*, indicating that higher poverty rates cause *lower* homicide rates! This is

contrary to a mountain of prior (and more sophisticated) research indicating that greater

economic deprivation causes higher homicide rates (for classic reviews, see Kovandzic,

Vieraitis, and Leisley 1998; and Land, McCall, and Cohen 1990). This bizarre finding is itself

strong reason to believe there was something seriously wrong with Webster et al.'s statistical

models. Likewise, Webster et al.'s analysis yielded a significant *positive* coefficient for bans on

"Saturday night special" handguns, indicating that these bans significantly *increases* the

homicide rate. While the National Rifle Association might welcome this finding, it is doubtful

that Webster et al. themselves would regard it as a plausible finding. Again, this dubious finding

points to the likelihood of errors in Webster et al.'s specification of their models, in particular the

omission of confounding variables.

Perhaps what is most conspicuous about Webster et al.'s statistical models, then, is the

completely arbitrary character of their choice of control variables - there is no evident rhyme or

reason to their choices. They include as controls variables that have been found in most prior

research to have no effect on crime rates (e.g., the number of law enforcement officers, official

unemployment rates, and bans on so-called "Saturday Night Specials"), while excluding

variables consistently found in prior research to affect homicide rates, such as the percent living

in urban or metropolitan areas, the African-American share of the population, and the divorce

rate. Note that Webster et al. did not say that they tested for effects of these variables and found

them unrelated to homicide rate; rather, there is no evidence that they ever included them in their

models in the first place. They did not even control for effects of other gun control laws, even

though Webster's prior writings make it amply clear that he believes many such laws reduce gun

crime, including not only PTP laws but also "assault weapon" bans, and gun registration laws

(see Webster, Vernick, McGinty, and Alcorn 2013).

It is almost as if Webster et al. were picking and choosing control variables on some basis

other than one grounded in their own empirical evidence, theory, or prior research. This is

especially worrisome, because it is possible to manipulate the estimated effect of a given variable

simply by failing to control for confounders. Confounders are, by definition, variables whose

control will affect estimates of the variable with which they are associated. That is, failing to

control for a genuine confounder will distort the estimate of the variable with which the

confounder is correlated. For example, Kleck (2018) reanalyzed the data underlying a study in

which the authors had found a large significant positive association between gun rates and

suicide rates (Miller et al. 2007), and showed that when five genuine confounders were

controlled that had not been controlled in the original analysis, the association initially observed

between guns and suicide disappeared. The original analysis had only controlled for, at most, a

single genuine confounder.

In the conclusions to their report, Webster et al. give the impression that they had ruled

out a substantial number of plausible alternative explanation of the MO post-repeal homicide

increase, listing no less than eight variables or categories of variables that could not explain this

increase. This listing is deceptive because few of these implied alternative explanations were

plausible in the first place, so ruling them out was a largely pointless exercise. The other factors

that Webster et al. claimed to have ruled out would not be considered by knowledgeable scholars

to be likely alternative explanations of this short-term homicide increase anyway, either because

the variables do not in general affect homicide rates (e.g., unemployment rates, as officially

measured; policing levels; MO's Stand Your Ground Law) or because have effects but do not

change enough over short periods of time to cause large short-term homicide increases. Because

they controlled for virtually no genuine confounders, their analyses could not rule out the

possibility that there was an outbreak of homicide in MO in 2008 that was entirely caused by

factors other than the PTP repeal.

   Sample Bias in the Analyses of Age-adjusted Homicide Rates

       Webster et al.'s reliance on age-adjusted firearms homicide rates derived from vital

statistics mortality data resulted in a biased sample of states, one systematically slanted to favor

the proposition that gun laws reduce homicide rates, or conversely that the absence or repeal of

gun laws increase homicide rates. The vital statistics data on which Webster et al. relied can be

obtained from the WONDER website of the Centers for Disease Control and Prevention (CDC

2018) but the CDC suppresses reporting of homicide data when there were fewer than ten

homicides in a given year in a given state. This had the effect of systematically excluding as

many as nine low homicide states from Webster et al.'s firearm homicide analyses: Delaware,

Hawaii, Maine, Montana, New Hampshire, North Dakota, South Dakota, Vermont, and

Wyoming (though Webster et al. claim to have used Delaware and Montana – see p. 295). These

nine states, not surprisingly, all have rates of total homicide and firearms homicide that are lower

than average (see, e.g. U.S., FBI 1998, pp. 76-87, 207). Further, with the exception of Hawaii

and Delaware, they also have less gun control than average (Brady Campaign 2013). States that

have little gun control yet nevertheless also have little homicide contradict the hypothesis that

less gun control causes more homicide. By excluding these contradictory states, Webster et al.

slanted their sample in favor of finding a negative association between the presence of gun laws

and homicide rates.

This sample bias was most serious for firearms homicide because state counts of gun

homicides are lower than total homicide counts, and thus more likely to be suppressed by CDC

policies. Unfortunately, Webster et al. chose not to make use of UCR data to measure firearms

homicide, even though the requisite data are reported for nearly all states in nearly all years in

the period 1999-2012 (see, e.g., U.S. FBI, 1998, pp. 68-74, 207). Their (p. 295) claims as to why

the UCR data cannot be used are without merit. They note that the UCR homicide rates are not

age-adjusted, but do not acknowledge that they did not need age-adjusted rates for their

purposes, since (a) they are nearly identical to non-adjusted homicide rates, and (b) state age

distributions do not change enough from year to year to have any detectable effect on homicide

rates. They accurately noted that the FBI has to perform interpolations for missing data from

nonreporting law enforcement agencies, but did not present any evidence that these procedures

introduce any significant errors in state homicide rates. And they certainly did not explain why it

makes sense to introduce massive sample bias into the study by omitting the 7-9 suppressed

CDC states altogether, all for the sake of avoiding purely hypothetical and probably minor

measurement flaws in the UCR homicide rates. In sum, Webster et al. needlessly used a severely

biased sample to analyze homicide rates when a relatively unbiased sample was available. They

did not say a word about the pronounced differences between the omitted states and those

included in their study sample regarding levels of gun control and homicide rates, or how their

use of vital statistics data biased their study sample.

Opinion 2

The study by Rudolph, Stuart, Vernick, and Webster (2015) of the impact of Connecticut's (CT) permit-to-purchase (PTP) law on homicide rates concluded that this measure, all by itself, "was associated with" an astounding 40% reduction in firearm homicide rates in the first 10 years after it was implemented. The authors' repeated references to the "effects" of the policy (e.g., see their Abstract, p. e49, or p. e53) make it clear that they did not interpret their finding as a mere statistical association, but rather regarded it as indicative of a causal effect of the law. This conclusion is not valid or scientifically based. Instead, their finding appears to be the result of (1) cherry-picking one particular change in gun law that was known *a priori* to be associated with a drop in gun violence, out of hundreds of gun law changes that might have been studied (as with the Missouri study), and (2) a poor application of the synthetic control methodology.

Arbitrary Cherry-Picking of One Particular Change in Gun Control Law

As previously noted here have been thousands of changes in gun control law, both increasing and decreasing the strictness of controls, in the 50 states and the District of Columbia (DC) in the past few decades. One might then reasonably ask, why did the authors study this one gun law change in this one place at this one time? The authors do not say. They were not commissioned by the state of CT to evaluate this particular, nor is the implementation of a PTP law on top of preexisting background checks an especially important change in gun law.

Indeed, it seems to be an especially unimportant change. Prospective gun buyers in CT already had to pass a background check to buy a handgun (or any other type of gun) even before CT's PTP law was enacted (due to the federal Brady Act), so the PTP law did not introduce

background checks. The PTP law changed background checks in one conceivably significant way, in that they were theoretically extended to cover handgun transfers by private sellers and not just dealer transfers. The authors do not, however, document even a single case of a prospective CT handgun buyer who was blocked by the PTP system from buying a handgun from a private seller, or provide any evidence that CT gun owners are even aware that background checks are required for private transfers. Thus, there is no affirmative evidence that this extension of background checks kept handguns away from anyone, never mind someone likely to commit a firearms homicide.

Less important elements of the CT PTP law were (1) the raising of the minimum age for buying a handgun from 18 to 20, and (2) requiring prospective handgun buyers to apply for a permit in person. The authors do not provide or cite any evidence that the latter provision has any additional effect above and beyond the effect of requiring a background check, or even any reasoning as to why it would be likely to discourage handgun acquisition more than the background check itself. As to former, research has already established that the increase in the minimum age for buying a handgun from 18 to 21 mandated by the federal Gun Control Act of 1968 had no measurable effect on rates of violent crime among persons age 18-20 (Kleck 2011).

In sum, there was no substantial *a priori* basis for expecting this particular minor change in CT's gun control regime to be especially significant, effective, or otherwise worth focusing research on. And even if there were some significant reason to focus on PTP laws in general, why focus only on CT's PTP in particular? The scientifically customary practice among the most sophisticated researchers in the field is to study *all* instances of a particular type of gun control law, since case studies of single examples have been found to be unreliable (Britt, Kleck, and Bordua 1996). Studies that cover all instances of a given type of gun law typically adopt

either of two methodological approaches: (1) a cross-sectional research design in which all jurisdictions with the law are compared with all those without it, using data pertaining to a single point in time, and statistically controlling for many other determinants of crime rates, or (2) a panel design with the same properties as the cross-sectional design, except using data covering multiple points in time. Rudolph et al. use neither approach.

The authors note that 10 states plus DC have PTP laws (p. e49). Why, then, study CT's PTP law in particular? The authors do not explain this seemingly arbitrary choice. These questions are crucial because if researchers decide to study just one instance of a policy that has been implemented in multiple jurisdictions, there is a risk that researchers will cherry-pick a single example that appears to support a preferred finding, even if analysis of all instances would have indicated that the policy was ineffective.

The danger can be illustrated by a simple example. Suppose gun control policy X had no effect whatsoever on homicide rates, but researcher W wanted to create the false impression that X was effective. This is easy to do with any widely implemented policy. In the long run, over the past half century or so, homicide rates have increased about half the time and decreased about half the time (U.S. Federal Bureau of Investigation 2017). Thus, at any one time that a violence control policy might be introduced, there is roughly a 50% chance that its introduction coincided with a reduction in the homicide rate – *even if the policy is completely ineffective.* All researcher W would need to do to create the false impression that some kind of gun control law was effective in reducing homicide would be to dredge through data on homicide rates in the 51 states (including DC), looking for declines in state firearms homicide rates occurring in any of the 50-some years for which state homicide statistics are available, and to then search for instances of new gun laws that happened to have been introduced in the same year. Given the

great frequency with which new gun laws are introduced (documented in connection with

Opinion 1), there would be hundreds of instances where introduction of a new gun law coincided

with a drop in firearms homicides, and an unscrupulous researcher could simply pick one of

them that showed an especially strong drop in the firearm homicide rate. He could then publish

the results of an analysis of firearms homicide trends in the single chosen state, focusing on the

change in the single arbitrarily selected type of gun control law as if it was the one specific type

of gun control that he wanted to evaluate all along. Indeed, it would be possible that policy X

was followed by homicide increases in every single state but one where it was implemented, and

that one state was the one that W analyzed.

There is no way to know if Rudolph et al. had any nefarious motives in selecting CT's

PTP law to analyze, but this is irrelevant. Even if their motives were pure, studying a single

arbitrarily selected implementation of a given type of widely implemented policy is an extremely

weak research design, prone to yielding misleading results for the foregoing reasons. As noted

in connection with Opinion 1, the last of the four authors of this study, Daniel Webster, has also

applied this unscientific research design to the repeal Missouri's PTP law, with similarly

unreliable findings.

Misapplication of the Synthetic Control Methodology

The synthetic control methodology itself, unlike the authors' focus on a single

implementation of a single type of gun control, is a potentially useful method for evaluating the

impact of a policy, if the method is applied properly. Unfortunately, these authors did not apply

the method properly.

The basic logic of the design is that the researcher looks for areas that, prior to the implementation of a new policy, had similar trends in the outcome variable (the homicide rate is this case) as well as correlates of the outcome variable. These areas are then combined into a single "synthetic control" unit whose trends in the outcome variable are used to simulate how that variable would have trended in the intervention area during the post-intervention period had that policy not been implemented. If post-intervention trends in the outcome variable are more favorable in the area with the new policy than in the synthetic control, the analyst tentatively concludes that the intervention was effective.

The effectiveness of the strategy depends entirely on how well the synthetic control (SC) simulates how homicide rates would have trended in CT had the PTP law not been introduced (the "counterfactual" situation). The measure of how well the SC is likely to do this is how closely the *pre*-law (before 1995) trends in firearms homicide rates and their correlates of the SC match the prelaw trends in CT. If this correspondence is poor, the method has no power to accurately estimate the impact of the PTP law because gun homicide trends in the SC area cannot tell the analyst how gun homicide rates would have trended in CT in the "counterfactual" situation where the PTP law was not passed.

The authors insist that their application of this method produced "the most accurate counterfactual" (p. e53), i.e. accurately indicated how CT's gun homicide rates would have trended after 1995 without the PTP law. As evidence of this supposed accuracy the authors report (p. e50) that the mean squared prediction error (MSPE) for their SC was optimized by the statistical algorithm used to select control states and assign weights to the data of each control states. The MSPE is a measure of how closely the prelaw trends in homicide and its correlates in "synthetic Connecticut" corresponded to CT's prelaw trends.

Assuming they were applied properly, the statistical procedures used by the authors do indeed select the best control states and weights to use to create the SC, within the limits of the available data. They do not, however, guarantee that the SC will be good. Instead, they may only yield a SC that is less bad than any alternatives. The MSPEs reported by the authors did not indicate that their SC effectively predicted what CT's homicide rates would have been in the absence of the PTP law; they only indicated that any other potential SCs would be worse.

The authors' only evidence bearing on whether their SC did a *good* job mimicking CT's pre-PTP homicide trends, as distinct from a merely a "less bad" job than other possible SCs, is shown in Figure 1. Visual inspection indicates that their SC ("synthetic Connecticut") performed very poorly indeed. That is, trends in pre-1995 firearm homicide rates in the SC corresponded very badly with pre-1995 trends in CT, indicating that the SC was unlikely to provide an accurate picture of how gun homicide would have trended in CT had the state not implemented its PTP program.

One can judge the quality of the authors' SC by noting how closely its pre-1995 trends in the firearms homicide rate correspond to CT's pre-1995 trends. By comparing the dashed line (representing the SC's trends) with the solid line (CT's trends) on the left half of the diagram, one can see that the correspondence is so poor that in many pre-1995 periods the SC *does not even trend in the same direction* as CT, never mind match CT's trends closely enough to serve the purposes of a synthetic control. For example, from 1984 to 1985, CT's actual gun homicide rate declined, but the SC's rate actually *increased*. From 1987 to 1988, CT's firearms homicide rate increased sharply, but the SC's rate declined. From 1989 to 1990, the SC's firearm homicide rate likewise moved in the opposite direction of CT's rate, as was true for the changes from 1990 to 1991, from 1991 to 1992, and from 1995 to 1996. There were 12 pre-1996 year-to-

year changes in firearms homicide trends, and the SC failed to even match the direction of CT's

changes in *six* of those 12 instances.

Figure 1 should have clearly alerted the researchers to the fact that their "synthetic

Connecticut" was not capable of accurately forecasting post-law trends in CT's gun homicide

rates in the absence of the PTP law, but they apparently failed to correctly interpret their own

diagnostic information. They appear not to have understood that their statistical procedures may

effectively select the *relatively* best set of control states and statistical weights, yet fail to

produce a good SC. Unfortunately, sometimes there simply are no useful control jurisdictions

available, and no amount of statistical manipulation of the data can create a SC that is capable of

accurately predicting post-intervention trends in the outcome variable.

Another key piece of information in Figure 1 was missed or ignored by the authors. The

upper-most line, consisting of alternating dots and dashes, represented trends in gun homicide in

all control states, i.e. states that did not implement a PTP law. This line showed that the firearms

homicide rate in states that did *not* implement a PTP law declined just as much as in CT,

supporting the interpretation that factors *other than PTP laws* were causing declines in gun

homicide after 1995.

Figure 1 also indicated that, even in CT, gun homicides had already been declining *before*

1995, beginning in 1993. Causation cannot run backwards in time, so CT's PTP law could not

have been responsible for these pre-1995 declines. The declines in CT after 1995 were little

more than a continuation of the downward trend that had already begun back in 1993, supporting

the interpretation that whatever *non-PTP* factors in CT that caused its decline in firearms

homicides between 1993 and 1995 continued to produce similar decreases after 1995.

Finally, Figure 1 showed that CT's homicide trends only became more favorable than those of the SC *four years* after the PTP law was implemented. That is, the timing of CT's improvement in homicide did not correspond with the introduction of the PTP system. The authors concede this (p. e51), but then try to downplay its significance by providing various speculative reasons why the PTP law would have delayed effects. They speculate that spikes in gun sales occurred prior to enactment of the PTP law, which temporarily counterbalanced the supposed beneficial effects of the law, but do not provide or cite any evidence that any such spike actually occurred. They also speculate that the number of transactions blocked by the PTP law had to accumulate over time before it would have any appreciable effect on handgun acquisition. This speculation is dubious for two reasons. First, the authors present no evidence that the PTP blocked any handgun acquisitions in the first place (beyond the effects produced by the preexisting background checks), so there is no reason to believe there was any "accumulation" of blocked handgun transactions. Second, the speculation is logically flawed. Even if the PTP law blocked handgun acquisitions that would not have been blocked by the preexisting background checks, one would have expected some mild effects as soon as the PTP law went into effect in 1995, even if its effects did grow over time. No such effect is evident in the authors' data for 1995, or for 1996, or for 1997, or for 1998 – CT's firearm homicide rate did not trend any more favorably than the SC's rate for 1995-1998 (Figure 1). Ignoring these dubious speculations, the one fact that we know for sure about the timing of CT's decline in firearm homicide is that *it did not correspond with the implementation of the PTP system.*

Summary: The authors data, as distinct from their highly selective interpretation of it, indicated that (1) the decline in firearms homicide rates in CT began years *before* the PTP law was implemented, (2) CT began to have a more favorable trend in firearms homicide than its

synthetic control only after four years had passed after the PTP law went into effect, and (3) CT's decline in firearms homicide after the PTP law went into effect in 1995 was no greater than that experienced by states that did not enact PTP laws. All these facts point to the same conclusion: the post 1995 decline in firearms homicides in CT was not caused by its enactment of a PTP law.

In conclusion, Rudolph et al. (1) misapplied the synthetic control methodology by failing to create an effective synthetic control, and (2) failed to recognize multiple strong indications in their own findings that strongly suggested that CT's PTP law had nothing to do with the post-1995 declines in the state's firearms homicide rate. Shorn of the invalid results of the SC analysis, the authors research provided no affirmative evidence that the minor new control elements introduced by CT's PTP system in 1995 had any measurable effect on firearms homicide rates above and beyond the effects of the background checks already in place before 1995.

The Claim that the PTP Law Reduced "Gun Diversion"

The authors argue that CT's PTP law somehow reduced the "diversion of guns to criminals" (p. e52). They did not present any evidence that actually supports this claim. Instead they assert that CT does a better job in preventing gun diversions and base this assertion on the fact that the average "sale-to-crime" interval for gun recovered by police is longer in CT than elsewhere. As previously noted in connection with Opinion 1, this interval (actually the time from retail sale to recovery by police) is *not* in any way an indicator of diversion of guns to criminals. To repeat, the assumption that a short time to recovery is an indicator of illegal gun diversion has long been discredited, and has never passed any empirical test of its validity (Kleck and Wang 2009, esp. pp. 1257-1263).

Opinion 3 – The Study by Crifasi et al. Had No Evidence Bearing on the

Impact of the FSA on the Supply of Crime Handguns in Baltimore

The study by Crifasi, Buggs, Choksy, and Webster (2017) alleges that Maryland's FSA caused a reduction in the supply of crime handguns in Baltimore (and Maryland as a whole), basing this claim on a misinterpretation of firearms trace data and *non sequitur* interpretations of a crude 4-question survey the authors conducted with a small unrepresentative sample of offenders.

The authors claim that one can somehow judge trends in the supply of crime handguns by examining firearms trace data on the guns recovered by police. The authors display a primitive, outdated understanding of the meaning and significance of trace data. They appear to be unaware that (1) the guns recovered by police are not representative of crime guns as a whole, or any subset of those guns, (2) samples of recovered crime guns overrepresent guns recovered by police soon after retail sale, (3) a short time-to-recovery (misdescribed by the authors as a short "time-to *crime*") is not an indicator that the recovered gun was "likely purchased with the intent of diverting that gun to a prohibited person" as the authors claim (p. 129), (4) the fact that a recovered gun was first sold at retail in a state different from the one in which it was recovered is not an indicator that it was trafficked or otherwise purchased with "the intent of diverting that gun to a prohibited person" (instead it usually indicates that the gun was stolen from a person who lawfully purchased the gun in one state, then moved their residence to the state in which police recovered the gun.

The authors claim (p. 129) that "a gun recovered with one year of retail sale indicates to law enforcement that the gun was *likely* purchased with the intent of diverting the gun to a

prohibited person," citing for support a 2002 ATF report. The claim is false, and the cited report

nowhere says that such a gun is *likely* to have been purchase for the person of diverting the gun

to a prohibited person. Note that the authors do not cite any specific page or quote any specific

passage in that ATF report to support their claim - this is because there is no such page or

passage. I have reviewed every published ATF report on trace data and have never seen any

claim that *most* recovered guns with a TTR under one year were guns purchased for purposes of

diversion. As far as ATF can tell, most of these firearms were legally purchased for lawful

purposes but then stolen from their lawful owners, usually in connection with a residential

burglary (Kleck and Wang 2009).

The authors repeatedly describe their findings about the set of guns they studies as if they

pertained to "Baltimore crime handguns" (e.g., p. 139). This is inaccurate. They studied the tiny

subset of handguns that had all of three attributes: (1) they were recovered by police, (2) they

were chosen by police to be submitted for tracing, and (3) they were successfully traced by ATF.

Such a sample is not representative of all crime handguns, or even of all those recovered by

police. The authors could hardly have honestly misunderstood this point, since it is forcefully

stated in no uncertain terms by ATF on the very first page of one of the reports the authors cite

(ATF 2016a): "Firearms selected for tracing are not chosen for purposes of determining which

types, makes or models of firearms are used for illicit purposes. The firearms selected *do not*

constitute a random sample and should not be considered representative of the larger universe of

all firearms used by criminals, or any subset of that universe" (ATF 2013, p. 1, emphasis added).

The authors response to ATF's warning was to ignore it and draw conclusions about Baltimore

"crime handguns" in general that made sense only if the authors rejected ATF's caveat. In fact,

the trace data examined by the authors can tell us nothing whatsoever about crime handguns in

Baltimore or Maryland – they describe only the unrepresentative subsets of crime handguns that police were able to recover, chose to submit requests on, and that ATF was able to successfully trace.

The problems with studying samples of successfully traced guns are not limited to the fact that they are not chosen in a way that guarantees they will be representative of all crime handguns. The problem is worse than that, since it is known that the processes of selection involved in generating these samples of guns systematically distort the samples in ways that overstate the share of guns that appear to have been purchased for purposes of diverting them into the hands of unqualified persons. Police are more likely to request traces on guns that appear to be new (and that therefore are likely to be less than a year old) because traces on such recently sold guns are more likely to generate usable leads pointing to criminals who possessed the guns recently. Further, ATF is more likely to be able to successfully trace recently sold guns since it is more likely that sales records still exist for newer guns (Kleck and Wang 2009, pp. 1271-1272). Samples of traced guns can also overstate the share of crime guns that had out-of-state origins. If police believe (correctly or not) that a large share of guns used in crimes in their state were smuggled in by interstate gun traffickers, this can lead them to be more likely to request traces on guns recovered from arrestees who seem more likely to be such a trafficker, such as persons with an out-of-state driver's license (pp. 1272-1273). In sum, the patterns in gun tracing data that the authors think reflect "illegal firearms diversion" actually reflect (a) the preferences of law enforcement officers for requesting traces on some guns and not others, and (b) the greater ability of ATF to successfully trace some guns and not others.

Did Firearms Availability to Criminals Decline After the FSA Went into Effect?

The "supply of crime handguns" is only relevant to the safety of Maryland's citizens if it affects the availability of handguns, or firearms in general, to criminals. The supply and availability are not the same. The total number of illegal handguns (the supply) might double while at the same time the share of criminals with access to guns remained the same or even declined. Thus, the authors never do directly address what really matters - whether availability of firearms to criminals decreased after the FSA became law.

The most direct measure of firearms availability among people willing to kill is the percent of homicides committed with guns (PHG). Note that this quantity does not measure the rate of homicide or the level of violence in general, since it could be quite low even if the homicide rate is high. For example, an area might experience many homicides but with only a small share of them being committed with guns. Conversely, an area might experience only a few homicides, but all of them committed with guns. Rather, PHG reflects, within the subset of the population who are willing to kill (whether that subset is large or small), the share who had access to a gun. To be sure, it does not measure *only* availability, since it is also influenced by the willingness of aggressors to use a gun in an attack, but it certainly does also reflect gun availability, since one obviously cannot commit a homicide with a gun unless one has access to a gun.

So what happened to the availability of guns to criminals in Maryland, as measured by this indicator, after the FSA went into effect on October 1, 2013? In the last three complete years *before* that, 2010-2012, **69.3%** (876 of 1264) of homicides were committed with firearms. In the first three complete years *after* the FSA, 2014-2016, **72.3%** of homicides were committed with firearms, indicating that *firearms availability among criminals willing to kill increased*

proportionally by 4.4% after the FSA became law (CDC 2018). This contradicts the thesis that the FSA reduced the availability of firearms to criminals in Maryland.

The Survey of Offenders

The authors also base their conclusions on the results of a survey of a nonprobability local sample of Baltimore probationers and parolees regarding their perceptions of whether it became harder to get a gun after the FSA went into effect. The authors concede that this convenience sample of criminals was not representative of criminals in general, in Baltimore or anywhere else (p. 132). For this reason alone we can conclude that the authors' findings can tell us nothing about whether the FSA actually made it harder for Baltimore (or Maryland) criminals in general to acquire guns.

The even more critical problem, however – not acknowledged by the authors – is that *they did not provide any evidence that the respondents (Rs) to this survey had any qualifications to make this judgement*. There is no evidence whatsoever that any of the Rs had actually tried to get a gun after the FSA went into effect, or had done so before it went into effect! And certainly, there was no evidence presented to indicate that any of the Rs had attempted to get a gun both before *and* after the FSA was implemented, providing them with some foundation in personal experience for a comparison on which they could base an assessment of whether it became harder for criminals to get guns after the FSA. More generally, the authors appear not to have asked any questions about their Rs actual recent experiences with guns. They instead seem to have simply assumed that just because their Rs were criminals, they must know something bearing on how easy it is to get guns.

Likewise, there was no evidence presented that any of these Rs had any personal experiences that could have informed them as to whether costs of guns increased after the FSA, whether others became less willing to buy guns on their behalf, or whether the FSA affected how easy it was to find a trusted person to sell them a gun. The authors did not establish that any of their Rs had *ever* asked another person to buy them a gun. Indeed, they did not establish that any of their Rs had ever even purchased a gun for themselves! The general lack of gun-related experiences among members of the authors' sample is suggested by their finding that 67% of the offenders had not carried or used a gun even a single time for any purpose in the six months preceding the interview (Table 5, p 137) – exactly what one expect if those offenders did not possess a gun at all. In this light, an even mildly skeptical observer might ask "How would any of these individuals be able to tell if it became harder to get a gun after the FSA went into effect? Most criminals are not gun criminals, so what relevance do the opinions of criminals without relevant gun-related experiences have to the issue of whether Baltimore's criminals found it harder to get a gun as a result of the FSA restrictions?"

Further, the way the authors asked their four questions violated fundamental rules of survey question wording, in a way that biased Rs' answers in a direction that favored the conclusions preferred by the authors. One of the most fundamental rules concerning the wording of either/or opinion questions is that one must "state the positive and negative side in the question stem" (Dillman, Smyth, and Christian, p. 134). For example, it is wrong to ask "Do you support the death penalty for persons convicted of murder?" The correct wording is "Do you support *or oppose* the death penalty …." Asking it the first way biases responses in favor of those expressing support for the death penalty, due to preexisting tendencies among some Rs to acquiesce with the sentiment seemingly held by the researchers.

In this light, consider the one-sided way the authors asked their first question: "Have the new gun laws made it more difficult to get a gun?" (Table 6, p. 138). A less biased wording of the question would have been "Have the new gun laws made it more difficult or less difficult to get a gun, or did it make no difference?" Thus, the authors biased the offenders' answers because they did not explicitly mention, in the stem of the question, the possibility of the laws making it *less* difficult to get a gun, and (2) they did not mention the possibility of the new laws having *no effect*.

These problems were aggravated by the incomplete set of response (answer) categories offered to Rs. The question was phrased as a yes/no question, so the only nonmissing answers (i.e., answers other than "don't know" or "refuse to answer") that Rs could provide were "yes" (implying that the news laws did make it harder to gun a gun) or "no," implying that it was not harder. This set of response options violated another basic rule that is learned even by beginning students of survey research methods: "Develop lists of answer categories that include all reasonable possible answers" (Dillman, Smyth and Christian, p. 135). The authors' incomplete set of answer categories made it impossible for Rs to explicitly indicate that they believed either (1) that the laws made it *easier* to get guns, or that (2) the new laws had *no effect*.

The authors found that 41% of the offenders answered the question "yes" (Table 6), and interpreted this as supporting their claim that the FSA did make it harder to get a gun (p. 138). This interpretation was a *non sequitur*. Given the flawed set of response categories offered to Rs, as far as the authors could determine, the 54% who answered "no" could have all believed that the law had made it *easier* to get a gun. If 41% of offenders thought the FSA made it harder to get a gun, but an even bigger 54% thought it made it easier, then overall the net effect of the

FSA, as perceived by offenders, was to *reduce* the difficulty of getting a gun – precisely the opposite of the interpretation imposed on their findings by the authors.

Even worse, the authors' second question, regarding cost of guns, did not even establish whether offenders thought the cost went up or down – the authors merely asked "Have the laws affected the cost?" The authors assumed that a "yes" response to this question obviously must have always indicated that the R thought the laws increased the cost, but that is just a guess. There is nothing in the wording of the question stem or the yes/no answer categories to establish the direction that Rs thought the cost of guns moved as a result of the new laws.

The authors' final two questions concerned the ease of making straw purchases of guns and of buying a gun from a trusted source – presumably one willing to sell to a buyer forbidden by law from buying a gun: "Have the laws affected the willingness of someone to buy a gun on your behalf?" and "Have the laws affected how easy it is to find someone you trust to sell you a gun?" (Table 6, p. 138). The questions were needlessly abstract and hypothetical, by not explicitly asking whether the offenders had *themselves*, after the FSA was implemented, *actually tried but failed* to enlist a straw purchaser to buy a gun for the offender, or had themselves *been unable to buy a gun* from a person they trusted. As far as the authors can tell from the responses to their survey, there was not a single offender in their sample who tried but failed to buy a gun as a result of the FSA.

In sum, due to the failure of the authors to study a representative sample of offenders, their flawed construction of question wordings and response categories, and their failure to inquire about actual experiences in trying to acquire guns, the results of their survey can tell us nothing about whether the Maryland FSA made it harder for criminals to get a handgun. To put it simply, they asked the wrong people the wrong questions.

Finally, it is worth noting that the report by Crifasi et al. had an odd focus, quite different from the other two studies Webster worked on. While those other two focused on the impact of changes in gun law on homicide rates, the Crifasi study says nothing at all about this topic, even though the presumed primary reason for reducing the supply of crime handguns in Baltimore was to reduce firearms violence, especially homicide, in Baltimore. Perhaps a clue to this curious choice of focus can be found in the statistics on Baltimore homicide. In the last three complete years before the FSA, 2010-2012, the homicide rate in Baltimore was 111.8 per 100,000 population. In the first three complete years after the FSA, 2014=2016, the rate was 152.0 per 100,000 (CDC 2018). Thus, *homicide increased by 36% after the FSA was in effect*. The riots associated with the police killing of Freddie Gray may well have contributed to part of this increase, but there is no evidence that it completely accounts for the homicide increase. In any case, the trends in Baltimore homicide certainly do not support the proposition that the FSA caused a reduction in Baltimore homicide rates.

Opinion 4 – The Irrelevancy of the Research on Which Webster Relies

Even if one ignored the many serious flaws in the studies cited by Daniel Webster, their findings would be irrelevant to the issue at stake in Maryland Shall Issue, Inc. et al. v. Hogan and Pallozzi - whether the HQL is likely to produce any public benefit that would compensate for the burdens it places on Maryland's citizens. None of these studies nor any others known to me were capable of separating the effects of the safety training elements in the HQL process from the effects of other elements of PTP laws. Even when conducted properly, studies of this type can only assess the overall global effect of PTP laws, not the effect of any one of their component elements.

It is rare that any law intended to reduce gun violence is composed of just a single pure element that is supposed to produce all of its beneficial effects. Legislators want to do whatever they can to reduce firearms violence, so it is understandable that the laws they create sometimes seem to "toss in everything but the kitchen sink" that might reduce violence. Unfortunately, this creates problems for those who want to know which, if any, of the multiple distinct elements of the new law produced benefits. This is important because some elements might be beneficial, others might have no effect, and still others might actually be harmful. If one could determine the separate effects of each element, one could amend the laws to delete the counterproductive and ineffective elements, retaining only the ones that produced benefits to the public that justified their costs.

The problem in the current case is that none of the studies of PTP laws known to me, including specifically the Missouri, Connecticut, and Baltimore studies cited by Daniel Webster, do anything to separate the effects of the sorts of elements that make up Maryland's HQL system from the numerous other, quite distinct, elements of PTP laws. Webster and his colleagues note that PTP laws in general may (1) extend background checks to cover private transfers, (2) require permit applicants to appear in person when applying, (3) require applicants to be fingerprinted, (4) increase the age at which one may purchase a handgun, *and* (5) require handgun safety training as in the HQL, along with a myriad of other possible elements (Webster et al. 2014; Rudolph et al. 2015). In addition, Maryland's FSA in particular (6) expanded the authority of state police to act against gun dealers found to have violated state gun laws, (7) required that gun owners report a lost or stolen gun to the authorities, (8) banned assault rifles, and (9) limited gun magazines to a maximum capacity of ten rounds (p. 130). In sum, there were at least nine

fundamentally different restrictions imposed on firearms by the FSA, each potentially having its own unique effect on firearms violence and criminal acquisition of firearms.

Nothing in the methods used by these scholars, or any other researchers known to me who have evaluated PTP laws, allowed them to separate the impact of the HQL-related elements from the effects of other elements of PTP laws. At no point does Webster explain how he could distinguish the effects of just one of the nine major elements of the FSA from the effects of the other eight elements. Thus, none of the research cited by Professor Webster is relevant to the issue of whether Maryland's HQL is likely to have any public safety benefits. Even if one believed that PTP laws as a whole reduced firearms violence, it would still be possible that HQL-style requirements were ineffective or even counterproductive, and that it was other elements in PTP laws that actually reduced gun violence.

Leaving aside the numerous and very diverse elements contained within the FSA, there were, by Webster's own admission, other policies implemented in Baltimore that were also intended to reduce gun violence, and that were operating at the same time that the FSA was in effect. The effective date of the FSA was October 1, 2013. Webster and his colleagues implied that they could somehow tell that the FSA was responsible for fewer Baltimore criminals having handguns after that date, yet according to a report by Webster, Buggs, and Crifasi (2018), a "Hot Spots" policing effort called the Violent Crime Impact Section (VCIS) was aimed at reduced gun violence, operated from 2007 through December 2012. If the effort reduced gun availability among violent people if could have had persisting effects for years after 2012. Webster, Buggs, and Crifasi concluded that this program was effective in reducing homicide (p. 10). Webster et al. do not explain how they could know that changes in "the supply of crime handguns in Baltimore" after June 2014 were due, even partially, to the FSA rather than the deterrent effect of

the VCIS in discouraging the selling, acquisition, or possession of handguns.   Webster et al.

(2018) also noted that the Baltimore Police Department operated specialized gun law

enforcement units such as the Special Enforcement Section (SES), which was deployed from

January 2013 to December 2014, and was specifically focused on reducing illegal gun possession

(Webster et al. 2018, p. 3), as well as a program of focused deterrence known as Group Violence

Intervention (GVI), which was also intended to reduce firearms violence, and was begun in June

2014.

Thus, Webster and his colleagues (1) did nothing to distinguish the effects of the safety

training requirements of the FSA from that law's other elements, and (2) did nothing that could

differentiate the effects of the FSA from other public policies that were also aimed at reducing

illegal gun possession and gun violence, and that were in operation in Baltimore at the same time

as the FSA. In sum, none of the research conducted or cited by Professor Webster can inform us

as to the effects of the FSA's safety training requirements on criminal possession or violent use

of firearms in either Baltimore or Maryland as a whole.

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1993   "Bad data and the 'Evil Empire': interpreting poll data on gun control." Violence
        and Victims 8(4):367-376.

1995   "Guns and violence: an interpretive review of the field." Social Pathology
        1(1):12-47.

1995   "Using speculation to meet evidence." Journal of Quantitative Criminology
        11(4):411-424.

1995   (with Marc Gertz) "Armed resistance to crime: the prevalence and nature of self-
        defense with a gun."   Journal of Criminal Law & Criminology 86(1):150-187.

1996   "Crime, culture conflict and sources of support for gun control: a multi-level
        application of the General Social Surveys." American Behavioral Scientist
        39(4):387-404.

1996   (with Chester Britt III and David J. Bordua) "A reassessment of the D.C. gun law:
        some cautionary notes on the use of interrupted time series designs for policy
        impact assessment." Law & Society Review 30(2):361-380.

1996   (with Chester Britt III and David J. Bordua) "Avoidance and misunderstanding."
        Law & Society Review 30(2):393-397.

1997   (with Marc Gertz) "The illegitimacy of one-sided speculation: getting the
        defensive gun use estimate down." Journal of Criminal Law and Criminology
        87(4):1446-1461.

1997   (with Tomislav Kovandzic and Marc Gertz) "Defensive gun use: vengeful
        vigilante imagery vs. reality: results from the National Self-Defense Survey."
        Journal of Criminal Justice 26(3):251-258.

1998   (with Marc Gertz) "Carrying guns for protection: results from the National Self-
        Defense Survey." Journal of Research in Crime and Delinquency 35(2):193-224.

1998   "What are the risks and benefits of keeping a gun in the home?" Journal of the
        American Medical Association 280(5):473-475.

1998   (with Charles Crawford and Ted Chiricos) "Race, racial threat, and sentencing of
        habitual offenders." Criminology 36(3):481-511.

1999   (with Michael Hogan) "A national case-control study of homicide offending and
        gun ownership." Social Problems 46(2):275-293.

1999   "BATF gun trace data and the role of organized gun trafficking in supplying guns
        to criminals." St. Louis University Public Law Review 18(1):23-45.

2001   "Can owning a gun really triple the owner's chances of being murdered?"
        Homicide Studies 5:64-77.

2002   (with Theodore Chiricos) "Unemployment and property crime: a target-specific
        assessment of opportunity and motivation as mediating factors."
        Criminology 40(3):649-680.

2004   "Measures of gun ownership levels for macro-level crime and violence research."
        Journal of Research in Crime and Delinquency 41(1):3-36.

2004   (with Jongyeon Tark) "Resisting crime: the effects of victim action on the
        outcomes of crimes." Criminology 42(4):861-909.

2005   (with Brion Sever, Spencer Li, and Marc Gertz) "The missing link in general
        deterrence research." Criminology 43(3):623-660.

2006   (with Jongyeon Tark and Jon J. Bellows) "What methods are most frequently
        used in research in criminology and criminal justice?" Journal of Criminal Justice
        34(2):147-152.

2007   "Are police officers more likely to kill African-American suspects?"
        Psychological Reports 100(1):31-34.

2007   (with Shun-Yung Wang and Jongyeon Tark) "Article productivity among the
        faculty of criminology and criminal justice doctoral programs, 2000-2005."
        Journal of Criminal Justice Education 18(3):385-405.

2008   (with Jongyeon Tark, Laura Bedard, and Dominique Roe-Sepowitz) "Crime
        victimization and divorce." International Review of Victimology 15(1):1-17.

2009   "The worst possible case for gun control: mass shootings in schools."
        American Behavioral Scientist 52(10):1447-1464.

2009   (with Shun-Yung Wang) "The myth of big-time gun trafficking and the
         overinterpretation of gun tracing data." UCLA Law Review 56(5):1233-1294.

2009   (with Tomislav Kovandzic) "City-level characteristics and individual handgun
        ownership: effects of collective security and homicide." Journal of Contemporary
        Criminal Justice 25(1):45-66.

2009   (with Marc Gertz and Jason Bratton) "Why do people support gun control?"
        Journal of Criminal Justice 37(5):496-504.

2011   (with James C. Barnes) "Article productivity among the faculty of criminology
        and criminal justice doctoral programs, 2005-2009." Journal of Criminal Justice

Education 22(1):43-66.

2011   (with Tomislav Kovandzic, Mark Saber, and Will Hauser). "The effect of
       perceived risk and victimization on plans to purchase a gun for self-protection."
       Journal of Criminal Justice 39(4):312-319.

2013   (with Will Hauser) "Guns and fear: a one-way street?" Crime and Delinquency
       59:271-291.

2013   "Gun control after Heller and McDonald: what cannot be done and what ought to
       be done." Fordham Urban Law Journal 39(5):1383-1420.

2013   (with J. C. Barnes) "Deterrence and macro-level perceptions of punishment
       risks: is there a "collective wisdom?" Crime and Delinquency 59(7):1006-1035.

2013   (with Tomislav Kovandzic and Mark Schaffer) "Estimating the causal effect of
       gun prevalence on homicide rates: A local average treatment effect
       approach." Journal of Quantitative Criminology 28(4):477-541.

2014   (with Jongyeon Tark) "Resisting rape: the effects of victim self-protection on
       rape completion and injury." Violence Against Women 23(3): 270-292.

2014   (with J. C. Barnes) "Do more police generate more crime deterrence?"
       Crime and Delinquency 60(5):716-738.

2015   "The impact of gun ownership rates on crime rates: a methodological review
       of the evidence." Journal of Criminal Justice 43(1):40-48.

2016   (with Tom Kovandzic and Jon Bellows) "Does gun control reduce violent
       crime? Criminal Justice Review 41:488-513.

2016   "Objective risks and individual perceptions of those risks." Criminology &
       Public Policy 15:767-775.

2016   (with Dylan Jackson) "What kind of joblessness affects crime? A national
       case-control study of serious property crime." Journal of Quantitative
       Criminology 32:489-513.

2016   "Large-capacity magazines and the casualty counts in mass shootings: the
       plausibility of linkages." Justice Research and Policy 17:28-47.

2017   (with Will Hauser) "The impact of police strength and arrest productivity on fear
       of crime and subjective assessments of the police" American Journal of Criminal
       Justice 42:86-111.

2017   (with Dylan Jackson) "Does crime cause punitiveness?" Crime & Delinquency.

63(12):1572-1599.

2017   (with Bethany Mims) "Article productivity among the faculty of criminology and criminal justice doctoral programs, 2010-2014." Journal of Criminal Justice Education 28(4):467-487.

2018   (with Moonki Hong) "The short-term deterrent effect of executions: an analysis of daily homicide counts." Forthcoming in Crime & Delinquency.

2018   "Response errors in survey estimates of defensive gun use." Published online in Crime & Delinquency, 3-26-18.

OTHER PUBLISHED ARTICLES

1985   "Policy lessons from recent gun control research." Law and Contemporary Problems 49(1):35-62.

1992   "Assault weapons aren't the problem." New York Times September 1, 1992, p. A15. Invited Op-Ed page article.

1993   "The incidence of violence among young people." The Public Perspective 4:3-6. Invited article.

1994   "Guns and self-protection." Journal of the Medical Association of Georgia 83:42. Invited editorial.

1998   "Using speculation to meet evidence: reply to Alba and Messner." Journal on Firearms and Public Policy 9:13-49.

1998   "Has the gun deterrence hypothesis been discredited?" Journal on Firearms and Public Policy 10:65-75.

1999   "There are no lessons to be learned from Littleton." Criminal Justice Ethics 18(1):2, 61-63. Invited commentary.

1999   "Risks and benefits of gun ownership - reply." Journal of the American Medical Association 282(2):136-136.

1999   "The misfire that wounded Colt's." New York Times October 23, 1999. Invited Op-Ed page article.

1999   "Degrading scientific standards to get the defensive gun use estimate down." Journal on Firearms and Public Policy 11:77-137.

2000   "Guns aren't ready to be smart." New York Times March 11, 2000. Invited Op-Ed page article.

2000   (with Chester Britt III and David J. Bordua) "The emperor has no clothes: using interrupted time series designs to evaluate social policy impact." Journal on Firearms and Public Policy 12:197-247.

2001   "School lesson: armed self-defense works." Wall Street Journal March 27, 2001. Invited opinion article.

2001   "Impossible policy evaluations and impossible conclusions: a comment on Koper and Roth." Journal of Quantitative Criminology 17:75-80.

2001   "Absolutist politics in a moderate package: prohibitionist intentions of the gun control movement." Journal on Firearms and Public Policy 13:1-43.

2002   "Research agenda on guns, violence, and gun control." Journal on Firearms and Public Policy 14:51-72.

2006   "Off target." New York Sun January 5, 2006. Invited opinion article.

2009   "How not to study the effect of gun levels on violence rates." Journal on Firearms and Public Policy 21:65-93.

2011   "Mass killings aren't the real gun problem --- how to tailor gun-control measures to common crimes, not aberrant catastrophes." Wall Street Journal January 15, 2011. Invited opinion article.

2011   "The myth of big-time gun trafficking." Wall Street Journal May 21, 2011. Invited opinion article.

2015   "Defensive gun ownership is not a myth: why my critics still have it wrong." Politico Magazine, February 17, 2015. Online at Politico.Com.

BOOK CHAPTERS

1984   (with David Bordua) "The assumptions of gun control." Pp. 23-48 in Don B. Kates, Jr. (ed.) Firearms and Violence: Issues of Regulation. Cambridge, Mass.: Ballinger.

(Also appeared in Federal Regulation of Firearms, report prepared by the Congressional Research Service, Library of Congress, for the Committee on the Judiciary, United States Senate, 1982).

1984   "The relationship between gun ownership levels and rates of violence in the U.S." Pp. 99-135 in Kates, above.

1984   "Handgun-only gun control: a policy disaster in the making." Pp. 167-199 in Kates, above.

1996   "Racial discrimination in criminal sentencing." Pp. 339-344 in Crime and Society, Volume III – Readings: Criminal Justice, edited by George Bridges, Robert D. Crutchfield, and Joseph G. Weis. Thousand Oaks, Calif.: Pine Forge Press.

1996   "Gun buy-back programs: nothing succeeds like failure." Pp. 29-53 in Under Fire: Gun Buy-Backs, Exchanges and Amnesty Programs, edited by Martha R. Plotkin. Washington, D.C.: Police Executive Research Forum.

2000   "Firearms and crime." Pp. 230-234 in the Encyclopedia of Criminology and Deviant Behavior, edited by Clifton D. Bryant. Philadelphia: Taylor & Francis, Inc.

2001   (with Leroy Gould and Marc Gertz) "Crime as social interaction." Pp. 101-114 in What is Crime?: Controversy over the Nature of Crime and What to Do About It, edited by Stuart Henry and Mark M. Lanier. Lanham, Md.: Rowman and Littlefield.

2003   "Constricted rationality and the limits of general deterrence." Chapter 13 in Punishment and Social Control: Enlarged Second Edition, edited by Thomas G. Blomberg. New York: Aldine de Gruyter.

2004   "The great American gun debate: what research has to say." Pp. 470-487 in The Criminal Justice System: Politics and Policies, 9th edition, edited by George F. Cole, Marc Gertz, and Amy Bunger. Belmont, CA: Wadsworth-Thomson.

2008   "Gun control." Article in The Encyclopedia of Social Problems, edited by Vincent N. Parrillo. Thousand Oaks, CA: Sage.

2009   "Guns and crime." Invited chapter. Pp. 85-92 in 21st Century Criminology: A Reference Handbook, edited by J. Mitchell Miller. Thousand Oaks, CA: Sage.

2012   Kovandzic, Tomislav, Mark E. Schaffer, and Gary Kleck. "Gun prevalence, homicide rates and causality: A GMM approach to endogeneity bias." Chapter 6, pp. 76-92 in The Sage Handbook of Criminological Research Methods, edited by David Gadd, Susanne Karstedt, and Steven F. Messner. Thousand Oaks, CA: Sage.

2012   (with Kelly Roberts) "What survey modes are most effective in eliciting self-reports of criminal or delinquent behavior?" Pp. 415-439 in Handbook of Survey Methodology, edited by Lior Gideon. NY: Springer.

2013   "An overview of gun control policy in the United States." Pp. 562-579 in The

Criminal Justice System, 10th edition, Edited by George F. Cole and Marc G. Gertz. Wadsworth.

2014 "Deterrence: actual vs. perceived risk of punishment. Article in Encyclopedia of Criminology and Criminal Justice. Berlin: Springer Verlag.

2018 "Gun control." Chapter in The Handbook of Social Control. New York: Springer. Forthcoming.

2018 "Guns and suicide." In Handbook on Gun Studies, edited by Jennifer Carlson, Kristin Goss, and Harel Shapira. NY: Routledge. Forthcoming.

BOOK REVIEWS

1978 Review of Murder in Space City: A Cultural Analysis of Houston Homicide Patterns, by Henry Lundsgaarde. Contemporary Sociology 7:291-293.

1984 Review of Under the Gun, by James Wright et al. Contemporary Sociology 13:294-296.

1984 Review of Social Control, ed. by Jack Gibbs. Social Forces 63: 579-581.

1985 Review of Armed and Considered Dangerous, by James Wright and Peter Rossi, Social Forces 66:1139-1140.

1988 Review of The Citizen's Guide to Gun Control, by Franklin Zimring and Gordon Hawkins, Contemporary Sociology 17:363-364.

1989 Review of Sociological Justice, by Donald Black, Contemporary Sociology 19:261-3.

1991 Review of Equal Justice and the Death Penalty, by David C. Baldus, George G. Woodworth, and Charles A. Pulaski, Jr. Contemporary Sociology 20:598-9.

1999 Review of Crime is Not the Problem, by Franklin E. Zimring and Gordon Hawkins. American Journal of Sociology 104(5):1543-1544.

2001 Review of Gun Violence: the Real Costs, by Philip J. Cook and Jens Ludwig. Criminal Law Bulletin 37(5):544-547.

2010 Review of Homicide and Gun Control: The Brady Handgun Violence Prevention Act and Homicide Rates, by J. D. Monroe. Criminal Justice Review 35(1):118-120.

LETTERS PUBLISHED IN SCHOLARLY JOURNALS

1987   "Accidental firearm fatalities." <u>American Journal of Public Health</u> 77:513.

1992   "Suicide in the home in relation to gun ownership." <u>The New England Journal of Medicine</u> 327:1878.

1993   "Gun ownership and crime." <u>Canadian Medical Association Journal</u> 149:1773-1774.

1999   "Risks and benefits of gun ownership." <u>Journal of the American Medical Association</u> 282:136.

2000   (with Thomas Marvell) "Impact of the Brady Act on homicide and suicide rates." <u>Journal of the American Medical Association</u> 284:2718-2719.

2001   "Violence, drugs, guns (and Switzerland)." <u>Scientific American</u> 284(2):12.

2002   "Doubts about undercounts of gun accident deaths." <u>Injury Prevention Online</u> (September 19, 2002). Published online at http://ip.bmjjournals.com/cgi/eletters /8/3/252.

2005   "Firearms, violence, and self-protection." <u>Science</u> 309:1674. September 9, 2005.

UNPUBLISHED REPORT

1987   <u>Violence, Fear, and Guns at Florida State University: A Report to the President's Committee on Student Safety and Welfare</u>. Reports results of campus crime victimization survey and review of campus police statistics on gun violence (32 pages).

RESEARCH FUNDING

1994   "The Impact of Drug Enforcement on Urban Drug Use Levels and Crime Rates." $9,500 awarded by the U.S. Sentencing Commission.

1997   "Testing a Fundamental Assumption of Deterrence-Based Crime Control Policy." $80,590 awarded by the Charles E. Culpeper Foundation to study the link between actual and perceived punishment levels.

PRESENTED PAPERS

1976   "Firearms, homicide, and the death penalty: a simultaneous equations analysis." Presented at the annual meetings of the Illinois Sociological Association, Chicago.

1979   "The assumptions of gun control." Presented at the Annual Meetings of the

American Sociological Association, New York City.

1980   "Handgun-only gun control: A policy disaster in the making." Presented at the Annual Meetings of the American Society of Criminology, Washington, D.C.

1981   "Life support for ailing hypotheses: Modes of summarizing the evidence on racial
discrimination." Presented at the Annual Meetings of the American Society of Criminology, Toronto.

1984   "Policy lessons from recent gun control research." Presented at the Duke University Law School Conference on Gun Control.

1985   "Policy lessons from recent gun control research." Presented at the Annual Meetings of the American Society of Criminology, San Diego.

1986   "Miscounting suicides." Presented at the Annual Meetings of the American Sociological Association, Chicago.

1987   (with Theodore G. Chiricos, Michael Hays, and Laura Myers) "Unemployment and crime: a comparison of motivation and opportunity effects." Annual meetings of the American Society of Criminology, Montreal.

1988   "Suicide, guns and gun control." Presented at the Annual Meetings of the Popular Culture Association, New Orleans.

1988   (with Susan Sayles) "Rape and resistance." Presented at the Annual Meetings of the American Society of Criminology, Chicago, Ill.

1989   (with Karen McElrath) "The impact of weaponry on human violence." Presented at the Annual Meetings of the American Sociological Association, San Francisco.

1989   (with Britt Patterson) "The impact of gun control and gun ownership levels on city violence rates." Presented at the Annual Meetings of the American Society of Criminology, Reno.

1990   "Guns and violence: a summary of the field." Presented at the Annual Meetings of the American Political Science Association, Washington, D.C.

1992 "Interrupted time series designs: time for a re-evaluation." Presented at the Annual Meetings of the American Society of Criminology, New Orleans.

1993   (with Chester Britt III and David J. Bordua) "The emperor has no clothes: Using interrupted time series designs to evaluate social policy impact." Presented at the Annual Meetings of the American Society of Criminology, Phoenix.

1993   "Crime, culture conflict and support for gun laws: a multi-level application of the General Social Surveys." Presented at the Annual Meetings of the American Society of Criminology, Phoenix.

1994   (with Marc Gertz) "Armed resistance to crime: the prevalence and nature of self-defense with a gun."   Presented at the Annual Meetings of the American Society of Criminology, Miami.

1995   (with Tom Jordan) "The impact of drug enforcement and penalty levels on urban drug use levels and crime rates." Presented at the Annual Meetings of the American Society of Criminology, Boston.

1996   (with Michael Hogan) "A national case-control study of homicide offending and gun ownership." Presented at the Annual Meetings of the American Society of Criminology, Chicago.

1997   "Evaluating the Brady Act and increasing the utility of BATF tracing data." Presented at the annual meetings of the Homicide Research Working Group, Shepherdstown, West Virginia.

1997   "Crime, collective security, and gun ownership: a multi-level application of the General Social Surveys." Presented at the Annual Meetings of the American Society of Criminology, San Diego.

1998   (with Brion Sever and Marc Gertz) "Testing a fundamental assumption of deterrence-based crime control policy." Presented at the Annual Meetings of the American Society of Criminology, Washington, D.C.

1998   "Measuring macro-level gun ownership levels." Presented at the Annual Meetings of the American Society of Criminology, Washington, D.C.

1999   "Can owning a gun really triple the owner's chances of being murdered?" Presented at the Annual Meetings of the American Society of Criminology, Toronto.

2000   "Absolutist politics in a moderate package: prohibitionist intentions of the gun control movement." Presented at the Annual Meetings of the American Society of Criminology, San Francisco.

2001   (with Tomislav V. Kovandzic) "The impact of gun laws and gun levels on crime rates." Presented at the Annual Meetings of the American Society of Criminology, Atlanta.

2001   "Measures of gun ownership levels for macro-level violence research." Presented at the Annual Meetings of the American Society of Criminology, Atlanta.

2002   "The effects of gun ownership levels and gun control laws on urban crime rates."
       Presented at the Annual Meetings of the American Society of Criminology,
       Chicago.

2003   (with Tomislav V. Kovandzic) "The effect of gun levels on violence rates depends
       on who has them." Presented at the Annual Meetings of the American Society of
       Criminology, Denver.

2003   (with KyuBeom Choi) "Filling in the gap in the causal link of deterrence."
       Presented at the Annual Meetings of the American Society of
       Criminology, Denver.

2004   (with Tomislav Kovandzic) "Do violent crime rates and police strength levels in
       the community influence whether individuals own guns?" Presented at the
       Annual Meetings of the American Society of Criminology, Nashville.

2004   (with Jongyeon Tark) "Resisting crime: the effects of victim action on the
       outcomes of crime." Presented at the Annual Meetings of the American
       Society of Criminology, Nashville.

2004   (with Jongyeon Tark) "The impact of self-protection on rape completion and
       injury." Presented at the Annual Meetings of the American Society of
       Criminology, Nashville.

2004   (with Kyubeom Choi) "The perceptual gap phenomenon and deterrence as
       psychological coercion." Presented at the Annual Meetings of the American
       Society of Criminology, Nashville.

2005   (with Jongyeon Tark) "Who resists crime?" Presented at the Annual Meetings of
       the American Society of Criminology, Toronto.

2005   (with Jongyeon Tark and Laura Bedard) "Crime and marriage." Presented at the
       Annual Meetings of the American Society of Criminology, Toronto.

2006   (with Shun-Yang Kevin Wang) "Organized gun trafficking, 'crime guns,' and
       crime rates." Presented at the Annual Meetings of the American Society of
       Criminology, Los Angeles.

2006   "Are police officers more likely to kill black suspects?" Presented at the Annual
       Meetings of the American Society of Criminology, Los Angeles.

2007   (with Shun-Yang Kevin Wang) "The myth of big-time gun trafficking. "Presented
       at the Annual Meetings of the American Society of Criminology, Atlanta.

2007   (with Marc Gertz and Jason Bratton) "Why do people support gun control?"

Presented at the Annual Meetings of the American Society of Criminology, Atlanta.

2008   (with J.C. Barnes) "Deterrence and macro-level perceptions of punishment risks: Is there a "collective wisdom?" Presented at the Annual Meetings of the American Society of Criminology, St. Louis.

2009   "The myth of big-time gun trafficking." Presented at UCLA Law Review Symposium, "The Second Amendment and the Right to Bear Arms After DC v. Heller." January 23, 2009, Los Angeles.

2009   (with Shun-Yung Wang) "Employment and crime and delinquency of working youth: A longitudinal study of youth employment." Presented at the Annual Meetings of the American Society of Criminology, November 6, 2009, Philadelphia, PA.

2009   (with J. C. Barnes) "Do more police generate more deterrence?" Presented at the Annual Meetings of the American Society of Criminology, November 4, 2009, Philadelphia, PA.

2010   (with J. C. Barnes) "Article productivity among the faculty of criminology and criminal justice doctoral programs, 2005-2009." Presented at the annual Meetings of the American Society of Criminology, November 18, 2010, San Francisco, CA.

2010   (with Will Hauser) "Fear of crime and gun ownership." Presented at the annual Meetings of the American Society of Criminology, November 18, 2010, San Francisco, CA.

2010   "Errors in survey estimates of defensive gun use frequency: results from national Internet survey experiments." Presented at the annual Meetings of the American Society of Criminology, November 19, 2010, San Francisco, CA.

2010   (with Mark Faber and Tomislav Kovandzic) "Perceived risk, criminal victimization, and prospective gun ownership." Presented at the annual Meetings of the American Society of Criminology, November 19, 2010, San Francisco, CA.

2011   (with Shun-young Wang) "The impact of job quality and career commitment on delinquency: conditional or universal?" Presented at the annual Meetings of the American Society of Criminology, November 17, 2011, Washington, D.C.

2011   (with Moonki Hong) "The short-term deterrent effect of executions on homicides in the United States, 1984-1998." Presented at the annual Meetings of the American Society of Criminology, November 16, 2011, Washington, D.C.

2011   (with Kelly Roberts) "Which survey modes are most effective in getting people

to admit illegal behaviors?" Presented at the annual Meetings of the American Society of Criminology, November 17, 2011, Washington, D.C.

2011   (with Will Hauser) "Pick on someone your own size: do health, fitness, and size influence victim selection?" Presented at the annual Meetings of the American Society of Criminology, November 18, 2011, Washington, D.C.

2011   (with Tomislav Kovandzic) "Is the macro-level crime/punishment association spurious?" Presented at the annual Meetings of the American Society of Criminology, November 18, 2011, Washington, D.C.

2012   (with Dylan Jackson) "Adult unemployment and serious property crime: a national case-control study." Presented at the annual Meetings of the American Society of Criminology, November 15, 2012, Chicago, IL.

2013   (with Will Hauser) "Confidence in the Police and Fear of Crime: Do Police Force Size and Productivity Matter?" Presented at the annual Meetings of the American Society of Criminology, November 22, 2013, Atlanta, GA.

2013. (with Dylan Jackson) "Adult unemployment and serious property crime: a national case-control study." Presented at the annual Meetings of the American Society of Criminology, November 22, 2013, Atlanta, GA.

2014   (with Dylan Jackson) "Does Crime Cause Punitiveness?" Presented at the annual Meetings of the American Society of Criminology, November 20, 2014, San Francisco, CA.

2015   "The effect of large capacity magazines on the casualty counts in mass shootings." Presented at the annual Meetings of the American Society of Criminology, November 18, 2015, Washington, D.C.

2015   (with Bethany Mims) "Article productivity among the faculty of criminology and criminal justice doctoral programs, 2010-2014." Presented at the annual Meetings of the American Society of Criminology, November 20, 2015, Washington, D.C.

2016   "Firearms and the Lethality of Suicide Methods." Presented at the annual Meetings of the American Society of Criminology, November 16, 2016, New Orleans, L.A.

2017   "Macro-level Research on the Effect of Firearms Prevalence on Suicide Rates: A Systematic Review and New Evidence." Presented at the annual Meetings of the American Society of Criminology, November 15, 2017.

CHAIR

1983   Chair, session on Race and Crime. Annual meetings of the American Society of Criminology, Denver.

1989   Co-chair (with Merry Morash), roundtable session on problems in analyzing the National Crime Surveys. Annual meetings of the American Society of Criminology, Reno.

1994   Chair, session on Interrupted Time Series Designs. Annual meetings of the American Society of Criminology, New Orleans.

1993   Chair, session on Guns, Gun Control, and Violence. Annual meetings of the American Society of Criminology, Phoenix.

1995   Chair, session on International Drug Enforcement. Annual meetings of the American Society of Criminology, Boston.

1999   Chair, Author-Meets-Critics session, More Guns, Less Crime. Annual meetings of the American Society of Criminology, Toronto.

2000   Chair, session on Defensive Weapon and Gun Use. Annual Meetings of the American Society of Criminology, San Francisco.

2002   Chair, session on the Causes of Gun Crime. Annual meetings of the American Society of Criminology, Chicago.

2004   Chair, session on Protecting the Victim. Annual meetings of the American Society of Criminology, Nashville.

DISCUSSANT

1981   Session on Gun Control Legislation, Annual Meetings of the American Society of Criminology, Washington, D.C.

1984   Session on Criminal Sentencing, Annual Meetings of the American Society of Criminology, Cincinnati.

1986   Session on Sentencing, Annual Meetings of the American Society of Criminology, Atlanta.

1988   Session on Gun Ownership and Self-protection, Annual Meetings of the Popular Culture Association, Montreal.

1991   Session on Gun Control, Annual Meetings of the American Statistical Association, Atlanta, Ga.

1995   Session on International Drug Enforcement, Annual Meetings of the American Society of Criminology, Boston.

2000   Session on Defensive Weapon and Gun Use, Annual Meetings of the American Society of Criminology, San Francisco.

2004   Author-Meets-Critic session on Guns, Violence, and Identity Among African-American and Latino Youth, by Deanna Wilkinson. Annual meetings of the American Society of Criminology, Nashville.

2007   Session on Deterrence and Perceptions, University of Maryland 2007 Crime & Population Dynamics Summer Workshop, Aspen Wye River Center, Queenstown MD, June 4, 2007.

2009   Session on Guns and Crime, at the DeVoe Moore Center Symposium On The Economics of Crime, March 26-28, 2009.

2012   Panel discussion of news media coverage of high profile crimes Held at the Florida Supreme Court On September 24-25, 2012, sponsored by the Florida Bar Association as part of their 2012 Reporters' Workshop.

PROFESSIONAL SERVICE

Editorial consultant -
American Sociological Review
American Journal of Sociology
Social Forces
Social Problems
Law and Society Review
Journal of Research in Crime and Delinquency
Social Science Research
Criminology
Journal of Quantitative Criminology
Justice Quarterly
Journal of Criminal Justice
Violence and Victims
Violence Against Women
Journal of the American Medical Association
New England Journal of Medicine
American Journal of Public Health
Journal of Homicide Studies

Grants consultant, National Science Foundation, Sociology Program.

Member, Gene Carte Student Paper Committee, American Society of Criminology, 1990.

Area Chair, Methods Area, American Society of Criminology, annual meetings in Miami, November, 1994.

Division Chair, Guns Division, American Society of Criminology, annual meetings in Washington, D.C., November, 1998.

Dissertation evaluator, University of Capetown, Union of South Africa, 1998.

Division Chair, Guns Division, American Society of Criminology, annual meetings in Washington, D.C., November, 1999.

Member of Academy of Criminal Justice Sciences selection committee for Editor of Justice Quarterly, 2007.

Outside reviewer of Dr. J. Pete Blair for promotion to Full Professor in the School of Criminal Justice at Texas State University, San Marcos, 2014.

UNIVERSITY SERVICE

Member, Master's Comprehensive Examination Committee, School of Criminology, 1979-1982.

Faculty Advisor, Lambda Alpha Epsilon (FSU chapter of American Criminal Justice Association), 1980-1988.

Faculty Senate Member, 1984-1992.

Carried out campus crime survey for President's Committee on Student Safety and Welfare, 1986.

Member, Strategic Planning and Budgeting Review Committee for Institute for Science and Public Affairs, and Departments of Physics and Economics, 1986.

Chair, Committee on Ph.D. Comprehensive Examination in Research Methods, School of Criminology, Summer, 1986.

Member, Committee on Ph.D. Comprehensive Examination in Research Methods, School of Criminology, Summer, 1986 to present.

Chair, Committee on Graduate Assistantships, School of Criminology, Spring, 1987.

Chair, Ad Hoc Committee on Computers, School of Criminology, Fall, 1987.

Member, Recruitment Committee, School of Criminology, Spring, 1988; Spring, 1989; and 1989-90 academic year.

Member, Faculty Senate Committee on Computer-Related Curriculum, Spring, 1988 to Fall, 1989.

Chair, Ad Hoc Committee on Merit Salary Distribution, School of Criminology, Spring, 1988.

Chair, Ad Hoc Committee on Enrollment Strains, Spring, 1989.

Member, Graduate Handbook Committee, School of Criminology, Spring, 1990.

Member, Internal Advisement Committee, School of Criminology Spring, 1990.

University Commencement Marshall, 1990 to 1993.

Member, School of Criminology and Criminal Justice Teaching Incentive Program award committee.

Chair, Faculty Recruitment Committee, School of Criminology and Criminal Justice, 1994-1995.

Chair, Committee on Ph.D. Comprehensive Examination in Research Methods, School of Criminology and Criminal Justice, 1994-1995.

Member, University Computer and Information Resources Committee, 1995-1998.

Member, University Fellowship Committee, 1995 to present.

Member, University Library Committee, 1996 to 1999.

Chair, Electronic Access Subcommittee, University Library Committee, 1998 to 1999.

Member, Ad Hoc Committee on Merit Salary Increase Allocation, School of Criminology and Criminal Justice, 1998-1999.

Member, Academic Committee, School of Criminology and Criminal Justice, 2000-present.

Member, Recruiting Committee, School of Criminology and Criminal Justice, 2000-2001.

Member, Promotion and Tenure Committee, School of Criminology and Criminal Justice, 2000-present.

Chair, Committee on Ph.D. Comprehensive Examination in Research Methods, School of Criminology and Criminal Justice, 2000-2002.

Chair, Promotion and Tenure Committee, School of Criminology and Criminal Justice, 2001-2002.

Faculty Adviser, School of Criminology and Criminal Justice Graduate Student Association, 2001-present.

Member, ad hoc committee on survey research, School of Criminology and Criminal Justice, 2002.

Coordinator of Parts 2 and 4 of the School of Criminology and Criminal Justice Unit Review, 2002.

Chair, Academic Committee, School of Criminology and Criminal Justice, 2002-2003.

Director, Honors Programs, School of Criminology and Criminal Justice, 2002-present.

Member, University Promotion and Tenure Committee, Fall, 2003 to present.

Member of University Graduate Policy Committee, Fall 2003 to present.

Director of Graduate Studies, School (later College) of Criminology and Criminal Justice, April 2004 to May 2011.

Chair, Promotion and Tenure Committee, College of Criminology and Criminal Justice, 2005-2006

Served as major professor on Area Paper by Christopher Rosbough, completed in 2012.

Served as member of dissertation committee of Kristen Lavin, dissertation completed in 2012.

Served as member of dissertation committee of Elizabeth Stupi, dissertation completed in 2013.

Served as outside member on two dissertation committees in 2014-2015: Brian Meehan in the Department of Economics and Adam Weinstein in the English Department. Both dissertations were completed.

Served as major professor on Area Paper on legalization of marijuana for Pedro Juan Matos Silva, Spring 2015. Paper completed.

Currently serving as major professor for two doctoral students, Moonki Hong and Sergio Garduno. Hong is scheduled to finish his dissertation by December 2015, and Garduno will be starting his dissertation in Spring 2016.

PUBLIC SERVICE

Television, radio, newspaper, magazine, and Internet interviews concerning gun control, racial bias in sentencing, crime statistics, and the death penalty. Interviews and other kinds of news media contacts include <u>Newsweek</u>, <u>Time</u>, <u>U.S. News and World Report</u>, <u>New York Times</u>, <u>Washington Post</u>, <u>Chicago Tribune</u>, <u>Los Angeles Times</u>, <u>USA Today</u>, <u>Boston Globe</u>, <u>Wall Street Journal</u>, <u>Kansas City Star</u>, <u>Philadelphia Inquirer</u>, <u>Philadelphia News</u>, <u>Atlanta Constitution</u>, <u>Atlanta Journal</u>, <u>Arizona Republican</u>, <u>San Antonio Express-News</u>, <u>Dallas Morning News</u>, <u>Miami Herald</u>, <u>Tampa Tribune</u>, <u>Jacksonville Times-Union</u>, <u>Womens' Day</u>,   <u>Harper's Bazaar</u>, <u>Playboy</u>, CBS-TV (60 Minutes; Street Stories) ABC-TV (World News Tonight; Nightline), NBC-TV (Nightly News), Cable News Network, Canadian Broadcasting Company, National Public Radio, Huffington Post, PolitiFact.com, and many others.

Resource person, Subcommittee on Crime and Justice, (Florida House) Speaker's Advisory Committee on the Future, February 6-7, 1986, Florida State Capitol.

Testimony before the U.S. Congress, House Select Committee on Children, Youth and Families, June 15, 1989.

Discussant, National Research Council/National Academy of Sciences Symposium on the Understanding and Control of Violent Behavior, April 1-4, 1990, Destin, Florida.

Colloquium on manipulation of statistics relevant to public policy, Statistics Department, Florida State University, October, 1992.

Speech to faculty, students, and alumni at Silver Anniversary of Northeastern University College of Criminal Justice, May 15, 1993.

Speech to faculty and students at Department of Sociology, University of New Mexico, October, 1993.

Speech on the impact of gun control laws, annual meetings of the Justice Research and Statistics Association, October, 1993, Albuquerque, New Mexico.

Testimony before the Hawaii House Judiciary Committee, Honolulu, Hawaii, March 12, 1994.

Briefing of the National Executive Institute, FBI Academy, Quantico, Virginia, March 18, 1994.

Delivered the annual Nettler Lecture at the University of Alberta, Edmonton, Canada, March 21, 1994.

Member, Drugs-Violence Task Force, U.S. Sentencing Commission, 1994-1996.

Testimony before the Pennsylvania Senate Select Committee to Investigate the Use of Automatic and Semiautomatic Firearms, Pittsburgh, Pennsylvania, August 16, 1994.

Delivered lectures in the annual Provost's Lecture Series, Bloomsburg University, Bloomsburg, Pa., September 19, 1994.

Briefing of the National Executive Institute, FBI Academy, Quantico, Virginia, June 29, 1995.

Speech to personnel in research branches of crime-related State of Florida agencies, Research and Statistics Conference, sponsored by the Office of the State Courts Administrator, October 19, 1995.

Speech to the Third Annual Legislative Workshop, sponsored by the James Madison Institute and the Foundation for Florida's Future, February 5, 1998.

Speech at the Florida Department of Law Enforcement on the state's criminal justice research agenda, December, 1998.

Briefing on news media coverage of guns and violence issues, to the Criminal Justice Journalists organization, at the American Society of Criminology annual meetings in Washington, D.C., November 12, 1998.

Briefing on gun control strategies to the Rand Corporation conference on "Effective Strategies for Reducing Gun Violence," Santa Monica, Calif., January 21, 2000.

Speech on deterrence to the faculty of the Florida State University School of Law, February 10, 2000.

Invited address on links between guns and violence to the National Research Council Committee on Improving Research Information and Data on Firearms, November 15-16, 2001, Irvine, California.

Invited address on research on guns and self-defense to the National Research Council Committee on Improving Research Information and Data on Firearms, January 16-17, 2002, Washington, D.C.

Invited address on gun control, Northern Illinois University, April 19, 2002.

Invited address to the faculty of the School of Public Health, University of Alabama, Birmingham, 2004.

Invited address to the faculty of the School of Public Health, University of Pennsylvania, March 5, 2004.

Member of Justice Quarterly Editor Selection Committee, Academy of Criminal Justice

Sciences, Spring 2007

Testified before the Gubernatorial Task Force for University Campus Safety, Tallahassee, Florida, May 3, 2007.

Gave public address, "Guns & Violence: Good Guys vs. Bad Guys," Western Carolina University, Cullowhee, North Carolina, March 5, 2012.

Invited panelist, Fordham Law School Symposium, "Gun Control and the Second Amendment,"   New York City, March 9, 2012.

Invited panelist, community forum on "Students, Safety & the Second Amendment," sponsored by the Tallahassee Democrat.

Invited address at University of West Florida, Department of Justice Studies, titled "Guns, Self-Defense, and the Public Interest," April 12, 2013.

Member, National Research Council Committee on Priorities for a Public Health Research Agenda to Reduce the Threat of Firearm-related Violence, May 2013.

Invited address at Davidson College, Davidson, NC, April 18, 2014. Invited by the Department of Philosophy.

OTHER ITEMS
Listed in:
Marquis Who's Who
Marquis Who's Who in the South and Southwest
Who's Who of Emerging Leaders in America
Contemporary Authors
Directory of American Scholars
Writer's Directory

Participant in First National Workshop on the National Crime Survey, College Park, Maryland, July, 1987, co-sponsored by the Bureau of Justice Statistics and the American Statistical Association.

Participant in Second National Workshop on the National Crime Survey, Washington, D.C., July, 1988.

Participant, Seton Hall Law School Conference on Gun Control, March 3, 1989.

Debater in Intelligence Squared program, on the proposition "Guns Reduce Crime." Rockefeller University, New York City, October 28, 2008. Podcast distributed through National Public Radio. Further details are available at
 http://www.intelligencesquaredus.org/Event.aspx?Event=36.

Subject of cover story, "America Armed," in <u>Florida State University Research in Review</u>, Winter/Spring 2009.

Grants reviewer, Social Sciences and Humanities Research Council of Canada, 2010.

Named one of "25 Top Criminal Justice Professors" in the U.S. by Forensics Colleges website (http://www.forensicscolleges.com/), 2014.

THE MYTH OF BIG-TIME GUN TRAFFICKING AND THE OVERINTERPRETATION OF GUN TRACING DATA

Gary Kleck[*]

Shun-Yung Kevin Wang[**]

In recent years the gun control movement has increasingly shifted its efforts from lobbying for new gun-control legislation to facilitating lawsuits against the gun industry, especially those based on claims of negligent distribution of firearms. These lawsuits are based on the premise that organized gun trafficking, much of it involving corrupt or negligent licensed dealers, plays an important role in supplying guns to criminals. This paper first assesses the extant evidence bearing on this claim, as well as on underlying assertions as to how one can tell whether a crime gun has been trafficked or whether a licensed dealer is involved in trafficking. Law enforcement evidence indicates that high-volume trafficking is extremely unusual, and that average "traffickers" handle fewer than a dozen guns. The aggregate volume of guns moved by known traffickers is negligible compared to even low estimates of the number of guns stolen.

City-level data on crime guns recovered in fifty large U.S. cities in 2000 are then analyzed to investigate (a) whether supposed indicators of gun trafficking are valid, (b) what factors affect trafficking levels, (c) the impact of gun trafficking on gun possession levels among criminals, and (d) the impact of gun trafficking on crime rates. The findings suggest that most supposed indicators that a crime gun has been trafficked have little validity. One possible exception is whether a gun has an obliterated serial number (OSN). Using the share of crime guns with an OSN as a city-level indicator of the prevalence of gun trafficking, the analysis showed that trafficking is more common where guns are scarcer. The analysis also showed that laws regulating the purchase of guns, including one-gun-a-month laws specifically aimed at trafficking, show no effect on trafficking activity. Finally, the research indicates that trafficking levels show no measurable effect on gun possession among criminals (measured as the share of homicides committed with guns), and generally show no effect on violent-crime rates.

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Exhibit B

INTRODUCTION

In recent decades the gun control movement has found it increasingly difficult to persuade legislatures to enact new restrictions on firearms. Republican dominance of state legislatures has reduced the chances of getting new state gun laws passed, and no new federal restrictions on guns of any significance have been enacted since the Brady Act was signed into law in 1994.[1] Shifts in the political winds have become so unfavorable that even previously pro-control political figures such as Barack Obama have deemphasized this issue and moved to assert their support for the Second Amendment and their belief in an individual right to keep and bear arms.[2]

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1.   U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, FEDERAL FIREARMS REGULATIONS REFERENCE GUIDE 2005 (2005), *available at* http://www.atf.gov/pub/fire-explo_pub/2005/p53004/index.htm.

2.   Robert D. Novak, *Obama's Second-Amendment Dance*, WASH. POST, Apr. 7, 2008, at A17; Organizing for Am., Urban Policy, http://origin.barackobama.com/issues/urban_policy/#crime-and-law-enforcement (last visited May 24, 2009).

As a result, the gun control movement has increasingly invested its efforts in alternative, nonlegislative strategies for advancing its cause. These include facilitating lawsuits by both governments and private parties against the gun industry in an attempt to gain in the courts what could not be gained in the legislature. In particular, the nation's leading gun control advocacy group, the Brady Center to Prevent Gun Violence, has through its Legal Action Project supported dozens of lawsuits by both private and public plaintiffs against the gun industry.[3] The suits are grounded in numerous legal rationales, but arguably the most important one, especially in suits aimed at manufacturers and distributors, is the claim that the industry engages in negligent distribution of firearms. For example, twenty-two of the first twenty-five suits brought by city, county, or state governments against manufacturers invoked claims of negligent distribution—the most common single claim in such suits.[4] Negligent distribution is presented by plaintiffs as an enabling tort in which noncriminal gun industry defendants cause third-party criminals to acquire guns and do harm with them. It is claimed that distributors and manufacturers are aware of widespread dealer misconduct, know who the bad dealers are, and could restrain their misconduct by denying them guns to sell or by forcing changes in the way they do business, if they chose to do so. Specifically, advocates assert that manufacturers and distributors could refuse to sell guns to "kitchen table" dealers who do not have stores, to those who sell guns at gun shows, or to those who sell multiple handguns at a time and who could train their employees to recognize attempts at straw purchases by gun traffickers or their confederates.[5] Advocates of these suits argue that they can motivate reform within the firearms industry, while opponents see them as a way of bankrupting the industry through ruinous legal expenses and damages.[6]

Lawsuits based on claims of negligent distribution, as well as those based on public nuisance theories, adopt a particular model of how guns move from lawful channels of commerce into the possession of criminals. According to this model, the prototypical movement of guns involves a gun trafficker, or a

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3. Brady Ctr. to Prevent Gun Violence, Legal Action Project, Gun Distribution & Sales, http://www.gunlawsuits.org/reform/distribution.php (last visited May 24, 2009).

4. Nat'l Rifle Ass'n Inst. for Legislative Action, Reckless Lawsuits: Taxpayer Funded Reckless Lawsuits Against the Firearms Industry, http://www.nraila.org/Issues/FactSheets/Read.aspx?id=147&issue=022 (last visited May 24, 2009).

5. *See, e.g.*, Mark Geistfeld, *Tort Law & Criminal Behavior (Guns)*, *in* GUNS, CRIME, AND PUNISHMENT IN AMERICA 384, 387 (Bernard E. Harcourt ed., 2003); David Kairys, *The Cities Take the Initiative*, *in* GUNS, CRIME, AND PUNISHMENT IN AMERICA *supra* at 363, 365.

6. *Compare* Brady Ctr. to Prevent Gun Violence, *supra* note 3, *with* Nat'l Rifle Ass'n Inst. for Legislative Action, *supra* note 4.

straw purchaser working for the trafficker, buying many or all of his guns from corrupt or negligent licensed gun dealers. Many traffickers supposedly purchase guns, especially handguns, in large batches from corrupt or irresponsible dealers, especially those operating in states with relatively weak controls over gun selling and buying. These guns are then moved to places with stricter local and state gun laws, where they are sold—supposedly at high markups—to criminal buyers.[7]

This image of illicit guns being smuggled from low-control states to high-crime cities with stricter controls is not put forward solely by gun control advocacy organizations. For example, at a 2007 NAACP presidential primary forum in Detroit, presidential candidate Barack Obama told his audience: "We've got to make sure that unscrupulous gun dealers aren't loading up vans and dumping guns in our communities, because we know they're not made in our communities. There aren't any gun manufacturers here, right here in the middle of Detroit."[8] Likewise, New York City mayor Michael Bloomberg clearly believes that corrupt or negligent out-of-state licensed gun dealers are substantially responsible for his city's gun violence problem.[9]

The federal Bureau of Alcohol, Tobacco, Firearms and Explosives (ATF), the Brady Campaign to Prevent Gun Violence, and some scholars have argued that gun traffickers are responsible for a significant share of the movement of guns into the hands of criminals, and that disrupting trafficking operations can therefore have a substantial impact on rates of criminal gun possession and gun violence.[10] This position depends for empirical support almost entirely on analyses of ATF gun-tracing data. So many tracing-based studies claiming to find support for this view have been published in recent

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7.   *See, e.g.*, BRADY CAMPAIGN TO PREVENT GUN VIOLENCE, LARGE VOLUME GUN SALES: THE ILLEGAL GUN TRAFFICKER'S BEST FRIEND, http://www.bradycampaign.org/pdf/faq/large-volume-sales.pdf (last visited May 24, 2009).

8.   On the Issues, Barack Obama on Gun Control, http://www.ontheissues.org/Domestic/Barack_Obama_Gun_Control.htm (last visited May 24, 2009).

9.   *See* Alan Feuer, *Gun Dealer and Mayor Face Showdown*, N.Y. TIMES, May 22, 2008, at B1.

10.   *See, e.g.*, Anthony A. Braga & Glenn L. Pierce, *Disrupting Illegal Firearms Markets in Boston: The Effects of Operations Ceasefire on the Supply of New Handguns to Criminals*, 4 CRIMINOLOGY & PUB. POL'Y 717 (2005); Anthony A. Braga et al., *The Illegal Supply of Firearms*, 29 CRIME & JUST. 319 (2002); Philip J. Cook et al., *Regulating Gun Markets*, 86 J. CRIM. L. & CRIMINOLOGY 59 (1995); David M. Kennedy et al., *Youth Violence in Boston: Gun Markets, Serious Youth Offenders, and a Use-Reduction Strategy*, LAW & CONTEMP. PROBS., Winter 1996, at 147; Christopher S. Koper, *Purchase of Multiple Firearms as a Risk Factor for Criminal Gun Use: Implications for Gun Policy and Enforcement*, 4 CRIMINOLOGY & PUB. POL'Y 749 (2005); Mark H. Moore, *Keeping Handguns From Criminal Offenders*, 455 ANNALS AM. ACAD. POL. & SOC. SCI. 92 (1981); Glenn L. Pierce et al., *Characteristics and Dynamics of Illegal Firearms Markets: Implications for a Supply-Side Enforcement Strategy*, 21 JUST. Q. 391 (2004); Franklin E. Zimring, *Street Crime and New Guns: Some Implications for Firearms Control*, 4 J. CRIM. JUST. 95 (1976).

decades that casual readers of the literature might conclude that a scholarly consensus has developed that organized gun trafficking is vital to the arming of America's criminals.[11]

We think that this notion deserves closer scrutiny. The goals of this paper are (1) to critically examine the existing evidence on the extent of organized or high-volume gun trafficking, (2) to evaluate the validity of using city-level traced-gun indicators to measure the prevalence of gun trafficking, and (3) to assess the effects of gun trafficking on criminal gun possession and crime rates.

I.   GUN TRAFFICKING AND THE FLOW OF GUNS TO CRIMINALS

The oft-stated assertion that gun traffickers supply many guns to criminals is trivial in the absence of any precise definition of a "gun trafficker." As used by ATF, the term refers to anyone who has ever unlawfully sold at least one gun.[12] Similarly, Anthony Braga and Glenn Pierce use the term "gun trafficking enterprises" to encompass operations that have unlawfully sold even a single gun.[13] The claim that there are many gun traffickers in this legalistic sense is unquestionably true, but largely devoid of policy implications. There is no doubt that unlawful selling of guns is commonplace in America, since gun theft is common, and most stolen guns are sold rather than kept by the thief.[14] Every thief who sells some of the guns he steals is a trafficker in this legalistic sense, even if he sells no more than one gun a year. James Wright and Peter Rossi estimate, from the sample of prisoners they interviewed, that felons who had ever stolen a gun had stolen an average of about thirty-nine guns in their lives[15]—fewer than four per year of their active criminal careers. As will be shown later, even the traffickers investigated by ATF sell, on average, fewer than fifteen guns over the entire course of their documented careers. Stopping even thousands of such occasional traffickers is unlikely to have much effect on the flow of guns to criminals, both because the share of "crime guns"[16] that any one of these criminals is responsible for is so small, and because such small-scale operators are so easily replaced. In any case, a policy redirecting significant law

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11.   See sources cited *supra* note 10.

12.   U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, CRIME GUN TRACE ANALYSIS REPORTS: THE ILLEGAL YOUTH FIREARMS MARKETS IN 27 COMMUNITIES 14 (1997).

13.   Braga & Pierce, *supra* note 10, at 726.

14.   *See* JAMES D. WRIGHT & PETER H. ROSSI, ARMED AND CONSIDERED DANGEROUS 199–204 (1986).

15.   *Id.* at 198.

16.   Crime guns are guns used to commit violent crimes, either in an attack or a threat.

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enforcement resources to such an effort probably could not be implemented in the first place—a point acknowledged even by advocates of greater efforts aimed at disrupting illegal gun markets.[17]

The issue of volume is crucial—the greater the number of guns sold by a trafficker, the more likely it is that stopping his activities will reduce the availability of guns to criminals. In this Article, we will use the term "high-volume gun trafficker" to denote a person who unlawfully and persistently sells substantial numbers of guns for profit. Any numerical threshold would be arbitrary—the underlying reality is that the more that flows of guns to criminals are concentrated in relatively few high-volume trafficking channels, the more impact one could realistically expect from a strategy of disrupting illicit suppliers. If pressed to state a number, however, we would regard a person who sold one hundred or more guns annually as a "large-scale" trafficker.

A.   Contrasting Models of the Movement of Guns to Criminals

It is critical for policy purposes to determine the degree to which the flow of guns to criminals is highly concentrated, moving through the hands of a relatively small number of high-volume illicit dealers (including both unlicensed dealers and corrupt or negligent licensed dealers). Such traffickers may be harder to quickly replace than occasional illicit sellers of guns, especially if the former make use of unusually rich criminal resources, including extensive contacts with a large customer base, organizations with large numbers of confederates, greater working capital, and greater skill in avoiding arrest. If such a trafficker were arrested and imprisoned, it would be less likely that he would be immediately replaced by an equally active substitute, such as a competitor or an associate in his own organization. On the other hand, if high-volume traffickers are rare and account for only a small share of illicit gun flow, such efforts are likely to be relatively unproductive because occasional illicit gun sellers are likely to be far more numerous and more quickly replaced.

ATF often states in its publications that gun traffickers supply a "significant" share of guns to criminals, without defining what "significant" really means. Many scholars have likewise claimed that criminals regularly involved in gun trafficking play an "important" role in channeling guns to criminals. These scholars have presented an image of relatively organized gun markets with significant numbers of high-volume traffickers, often oper-ating in concert with corrupt or irresponsible licensed dealers who provide

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17.   Pierce et al., *supra* note 10, at 420.

the traffickers with their supply of guns.[18]   Typical of such scholars, Philip Cook and Anthony Braga concede that diffuse (low-volume) sources channel many guns to criminals, but nevertheless insist that point sources (high-volume traffickers) are important in supplying guns to criminals.[19]

This concentrated gun trafficking model holds that a significant share of guns are diverted from lawful commerce into the hands of criminals by the illegal activities of corrupt or negligent federal firearms licensees (FFLs) and unlicensed, criminal gun traffickers.   A prototypical point-source trafficker, according to this model, obtains many or all of his guns from corrupt or careless FFLs, who either sell guns directly to the trafficker in unrecorded transfers or make recorded sales to straw purchasers—legally qualified persons who purchase guns on behalf of another person.   Many traffickers, according to this model, purchase guns—especially handguns—in large batches from corrupt or irresponsible dealers, especially those operating in states with relatively weak controls over gun selling and buying.   This model is preferred by advocates of supply-side gun control strategies, since it promises significant reductions in criminal gun possession if high-volume traffickers or corrupt dealers can be stopped.[20]

The case for the concentrated model relies heavily on vague claims about the significant amount of illegal diversion of guns by gun traffickers (very broadly defined) operating in illicit gun markets.   Pierce and his colleagues provide a good example: "Our results indicate that a noteworthy percentage of the guns recovered in crime come rather directly from licensed dealers; in effect criminals are being supplied by dedicated 'pipelines' as well as the extant pool of guns."[21]   Nothing in the authors' results points to even an approximation of what this noteworthy percentage might be.   The only percentages the authors cite pertain to the share of crime guns that possess

18.   Braga et al., *supra* note 10, at 319–52; Philip J. Cook & Anthony A. Braga, *Comprehensive Firearms Tracing: Strategic and Investigative Uses of New Data on Firearms Markets*, 43 ARIZ. L. REV. 277–309 (2001); Cook et al., *supra* note 10, at 59–92; Kennedy et al., *supra* note 10, at 147–96; Moore, *supra* note 10, at 92–109; Koper, *supra* note 10, at 749–78; Pierce et al., *supra* note 10, at 391–422; Zimring, *supra* note 10, at 95–107.

19.   Cook & Braga, *supra* note 18, at 308.

20.   U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, COMMERCE IN FIREARMS IN THE UNITED STATES—FEBRUARY 2000, at 11–12, 17–25 (2000); U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, CONCENTRATED URBAN ENFORCEMENT (1977) [hereinafter CONCENTRATED URBAN ENFORCEMENT]; U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, FOLLOWING THE GUN: ENFORCING FEDERAL LAWS AGAINST FIREARMS TRAFFICKERS, 10–22 (2000) [hereinafter FOLLOWING THE GUN]; U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, GUN SHOWS: BRADY CHECKS AND CRIME GUN TRACES, JANUARY 1999 (1999); Braga et al., *supra* note 10; Cook & Braga, *supra* note 18; Cook et al., *supra* note 10; Kennedy et al., *supra* note 10; Pierce et al., *supra* note 10.

21.   Pierce et al., *supra* note 10, at 419.

various ambiguous characteristics believed to be indicators of trafficking, such as rapid movement of guns from first retail sale to recovery by police in connection with a crime. The authors report that "nearly a third" of their traced guns had two or more of ten purported indicators of gun trafficking, and hint that guns with this many indicators were likely to have been trafficked, but provide no evidence of this.[22] They do not explain why having just two of these ambiguous indicators should be regarded as strong evidence that a gun was trafficked. None of their findings suggest that even 1 percent of crime guns had as many as half of the ten indicators that they considered.[23]

Pierce and his colleagues assert that "a supply-side gun market disruption strategy focused on quick diversions of guns from federally licensed dealers may prove to be particularly fruitful" in some cities.[24] It becomes evident how vague this assertion is once one realizes that quick diversions from FFLs include not only purchases by traffickers and straw purchasers, but also relatively new guns stolen from their lawful buyers, one or two at a time, in burglaries—diversions beyond the control of either FFLs or ATF. The authors do not provide any specific examples of gun market disruption strategies that would reduce the rate of burglary-linked gun thefts, nor do they provide any evidence to contradict the hypothesis that nearly all quick diversions are the result of gun thefts from lawful buyers rather than of organized gun trafficking.

Advocates of the concentrated gun trafficking model have never stated, in even the most approximate terms, what they mean by a significant share of crime guns being trafficked. They have never explicitly claimed, for example, that even as much as a tenth of crime guns are trafficked. They only assert that high-volume point sources are important in supplying guns to criminals,[25] and they make it clear that they believe the trafficked share is large enough to justify the investment of more law enforcement resources focused on high-risk retail dealers and unlicensed traffickers.[26]

The contrasting dispersed-gun-flow model assumes a highly dispersed market in which criminals obtain guns from a wide variety of largely interchangeable nontrafficker sources. In this view, criminals most commonly (1) obtain guns (directly or indirectly) as a by-product of thefts, primarily

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22.   *Id.* at 419.
23.   *Id.* at 417.
24.   *Id.* at 418.
25.   *E.g.*, Cook & Braga, *supra* note 18, at 308.
26.   Braga et al., *supra* note 10; Pierce et al., *supra* note 10; D.W. Webster et al., *Effects of Undercover Police Stings of Gun Dealers on the Supply of New Guns to Criminals*, 12 INJ. PREVENTION 225 (2006).

residential burglaries, that were not committed specifically for the purpose of obtaining guns; (2) buy guns one at a time from friends and relatives who neither regularly sell guns nor act as straw purchasers; or (3) (if they have no criminal convictions) lawfully purchase guns from licensed dealers, to whom they are indistinguishable from noncriminal buyers. According to this model, high-volume or persistent traffickers are rare, and in the aggregate are of little significance in the arming of criminals. Those who sell guns illegally are not professionals, specialists, or part of criminal organizations devoted to gun trafficking, and they do not sell guns persistently or in large numbers. Illicit gun sellers are instead more likely to be thieves who sell a few guns (typically fewer than a half-dozen per year) along with all the other saleable property they steal, drug dealers who occasionally sell guns as a sideline to their drug business, or friends and relatives of the criminal recipient who do not regularly sell guns.[27]

Thus, while many crime guns are supplied by black market or street sources, almost all of these are casual low-volume suppliers rather than high-volume point sources. Those holding to this model recognize that some criminals acquire guns legally from licensed dealers through legal purchases (because the criminals are not convicted felons, and do not show up as hits in background checks), while others may use straw purchasers to illegally buy guns from licensed retailers who have no way of recognizing the putative buyers as straws. But the model denies that either intentional criminal conduct or carelessness on the part of licensed retailers contributes significantly to such diversion of guns to criminals, or that such acquisitions are typically part of repeated efforts by traffickers to acquire guns to resell for profit. Instead, the dispersed flow model implies that people who act as straws for ineligible buyers do so only once or very rarely, rather than repeatedly on behalf of traffickers intent on accumulating a supply of guns to sell for profit.

William Vizzard, a political scientist who also served for twenty-seven years as an ATF agent, summarized his view of gun trafficking:

> Nothing in the available studies supports an assumption of a well-structured illicit market in firearms. Transactions appear to be casual and idiosyncratic. My own experience, and that of most other agents I have interviewed, supports an assumption that the majority of sources is very dispersed and casual, and regular traffickers in firearms to criminals are few.[28]

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27.   *See* JOSEPH SHELEY & JAMES D. WRIGHT, IN THE LINE OF FIRE 46–51 (1996); WRIGHT & ROSSI, *supra* note 14, at 184–87, 196, 198, 202–04; Gary Kleck, *BATF Gun Trace Data and the Role of Organized Gun Trafficking in Supplying Guns to Criminals*, 18 ST. LOUIS U. PUB. L. REV. 23, 39–40 (1999).

28.   WILLIAM VIZZARD, SHOTS IN THE DARK 31 (2000).

Vizzard attributed the rarity of "regular traffickers in firearms" to the huge reservoir of guns in the United States, and the concomitant fact that criminals can easily draw on many different sources for guns. The existence of these conditions suggests that "there is little economic incentive for persons to specialize in the illegal gun trade."[29] His discussion, however, leaves open the possibility that there could be such specialists in a few exceptional places, such as New York City, where gun laws are exceptionally restrictive and alternative sources of guns are unusually limited. It further leaves open the possibility that some criminals, such as drug dealers, might illegally sell a fairly large number of guns even though they do not specialize in the activity.[30]

B.   The Scale of the Total Flow of Guns to Criminals

It is impossible to meaningfully judge whether the volume of guns moved into criminal hands through a given channel is significant without at least a rough sense of the total volume of guns acquired by criminals. A conservative estimate of the number of guns acquired by criminals can be obtained by beginning with estimates of the number of guns stolen each year, and then extrapolating that number to the total number of guns obtained by all methods, based on the share of their guns that criminals say they obtain by theft.[31] The best available estimate of the number of annual gun theft incidents comes from the National Crime Victimization Survey (NCVS), which collects data on thefts, including incidents not reported to the police. The survey indicated that in the calendar year 2000 there were 174,680 gun theft incidents that people were willing to report to its interviewers,[32] while the figure for 1993—a higher crime year—was 291,820.[33] These estimates are almost certainly conservative because people are reluctant to report thefts of guns that they possess illegally, or whose legal status they are unsure of. The NCVS does not establish the number of guns stolen per incident. The largest national survey to estimate this parameter found that there were 2.2 guns

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29.   *Id.*

30.   WRIGHT & ROSSI, *supra* note 14, at 203–04.

31.   Kleck, *supra* note 27, at 40–41.

32.   BUREAU OF JUSTICE STATISTICS, U.S. DEP'T OF JUSTICE, CRIMINAL VICTIMIZATION IN UNITED STATES, 2000 STATISTICAL TABLES, tbl.84, *available at* http://www.ojp.gov/bjs/pub/pdf/cvus00.pdf (last visited May 27, 2009).

33.   BUREAU OF JUSTICE STATISTICS, U.S. DEP'T OF JUSTICE, CRIMINAL VICTIMIZATION IN UNITED STATES, 1993 STATISTICAL TABLES, tbl.84, *available at* http://www.ojp.usdoj.gov/bjs/pub/pdf/cvus935.pdf (last visited May 27, 2009).

stolen per gun theft incident.[34] Thus, a conservative estimate of the number of guns stolen in 2000 would be 384,296, while the figure for 1993 would be 642,000. The NCVS's data indicate that about 53 percent of stolen guns are handguns,[35] and thus imply that at least 203,677 handguns were stolen in 2000, and 340,260 in 1993.

The most extensive questioning of criminals on the sources of their guns indicated that felons had personally stolen 32 percent of their most recently acquired handguns.[36] This implies that the total number of handguns acquired by criminals is about 3.125 times larger than the number of handguns stolen, and thus that about 636,490 handguns were acquired by criminals by all methods in 2000, and about 1.1 million in 1993. If the percent of all types of guns acquired by theft was the same as for handguns, these figures would imply that criminals acquired about 1.2 million guns of all types 2000 and about 2.0 million in 1993. On the other hand, if one accepts at face value, as some scholars apparently do,[37] the results of a 1997 federal survey of prison inmates who used or possessed a firearm during their current offense, which indicated that only 10 percent of criminals' handguns were acquired by theft,[38] then the total number of guns acquired by criminals each year would necessarily be ten times as large as the number they stole—about 3.8 million in 2000 and 6.4 million in 1993. We regard such huge figures as implausible, and believe it is unlikely that inmates were fully reporting their gun theft activity to the federal government interviewers. If the ten-percent figure is a product of underreporting, then the theft share would be over ten percent, and the total number acquired by all means would be less than ten times the number stolen. In any case, even conservative estimates indicate that the number of handguns annually obtained by criminals by all methods exceeds 600,000 even in low-crime years. And since handguns claim only half of the guns obtained by criminals via theft, if the same applies to all methods of acquisition, criminals obtain, by all methods, at least 1.2 million guns of all types each year.

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34.   PHILIP J. COOK & JENS LUDWIG, GUNS IN AMERICA 30 (1996).
35.   BUREAU OF JUSTICE STATISTICS, U.S. DEP'T OF JUSTICE, GUNS AND CRIME 2 (1994).
36.   WRIGHT & ROSSI, *supra* note 14, at 184.
37.   *See, e.g.*, Braga et al., *supra* note 10, at 328.
38.   BUREAU OF JUSTICE STATISTICS, U.S. DEP'T OF JUSTICE, FIREARM USE BY OFFENDERS 6 (2001).

C.   Law Enforcement Evidence on the Prevalence and Volume of
     Gun Trafficking

The most direct, albeit limited, evidence on the extent of significant organized gun trafficking is law enforcement information gathered in connection with the investigation of traffickers. As with many other types of criminals, much of what we know about gun traffickers is based on those who are arrested. Christopher Koper and Peter Reuter uncritically cite the assessment of unnamed federal officials that a gun running operation that handled 116 guns was "typical of the size of most gun running operations."[39] However, traffickers handling this many guns are extremely rare among those caught by law enforcement, and a more typical volume would be fifteen or fewer guns sold per year.[40] Although ATF places a high priority on catching high-volume traffickers,[41] the agency was able to identify, over a two-and-a-half-year period (1996–1998), just thirty-seven trafficking operations in the United States in which over 250 guns were trafficked. Thus, on average, there were fewer than fifteen high-volume trafficking operations uncovered by ATF per year in the entire nation.[42] Further, ATF uncovered only 104 trafficking operations that handled over a hundred guns, or about forty-two such operations per year.[43] Thus, by any reasonable standard, ATF rarely uncovers large-scale gun trafficking operations.

It is possible, however, that local law enforcement agencies uncover many additional high-volume dealers, especially in places where political leaders prioritize going after gun trafficking. If big-time traffickers operate anywhere, one would expect to find them in New York City, given its huge size (and correspondingly large number of potential customers), its low level of legal handgun ownership, and its strict gun laws, which reduce the availability of legal handguns. Assuming that law enforcement agencies like to publicize their major successes, higher-volume trafficking cases should be reported in local newspapers once investigations are complete. However, an examination of all New York City daily papers over a 17-year period from 1990 through 2006 uncovered just six cases of trafficking operations purportedly involving a hundred or more guns, or about one such operation

39.   Christopher S. Koper & Peter Reuter, *Suppressing Illegal Gun Markets: Lessons From Drug Enforcement*, LAW & CONTEMP. PROBS., Winter 1996, at 119, 127.

40.   U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, DEP'T OF THE TREASURY, CRIME GUN TRACE REPORTS (2000): NATIONAL REPORT 53 (2002) *available at* http://www.atf.gov/firearms/ycgii/2000/index.htm.

41.   U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, *supra* note 12, at 2.

42.   FOLLOWING THE GUN, *supra* note 20, at 7, 24.

43.   *Id.*

reported every three years in the nation's largest city.[44] Only two of these operations were alleged to have trafficked over 140 guns.[45]

Likewise, in Chicago, which like New York City bans the private possession of handguns, the police catch virtually no high-volume gun traffickers. A newspaper story clearly intended to convey the idea that interstate gun traffickers were important in supplying guns to Chicago criminals nevertheless identified only two traffickers who dealt in even modest numbers of guns—ninety-five and thirty-five guns, respectively.[46] To put this in perspective, these two traffickers were arrested in a year (2003) in which the Chicago police seized over 10,000 guns from criminals.[47] If high-volume gun traffickers are almost never uncovered in the nation's largest cities with the strictest controls on handguns, it is highly unlikely that local police in areas with weaker gun controls discover significant numbers of such traffickers, where there would be less need for their services.

These few high-volume operations are clearly the well-publicized exceptions, since average trafficking operations involve far fewer guns. In 2000, ATF initiated 1,319 trafficking investigations and estimated that the targeted operations had trafficked a total of 19,777 firearms, for an average of just fifteen guns per trafficking operation.[48] Arithmetic means, however, are misleading, with highly skewed distributions such as these in which a handful of operations handling extremely large numbers of guns drive up the average. It follows that the median number of guns trafficked per operation is less than half the average,[49] so a typical operation (one with a median volume) investigated in 2000 probably handled fewer than seven guns. Further, the average gun volume among *all* trafficking operations, including those not important enough to merit ATF investigation, would almost certainly be lower still. Although investigators may underestimate the number of the guns trafficked, the number that has been documented is clearly small. It also should be kept in mind that traffickers sell to virtually anyone with money, not just criminals, so the number of guns going to criminals is necessarily smaller than the total number trafficked.[50]

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44.   This result was drawn from a LexisNexis search of all New York City daily papers for "gun trafficking," "gun smuggling," or "gun running."

45.   *Id.*

46.   David Heinzmann, *Gangs Run Gun Pipeline From Delta to Chicago—Lenient Laws Make Buying Weapons Easier in South*, CHI. TRIB., Feb. 5, 2004, at 1.

47.   *Id.*

48.   U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, *supra* note 40, at 53.

49.   FOLLOWING THE GUN, *supra* note 20, at 13.

50.   VIZZARD, *supra* note 28, at 31.

What share of all guns acquired by criminals is supplied, then, by known traffickers? As noted above, the total number of guns known to have been trafficked by all traffickers investigated by ATF in 2000 was 19,777. We have estimated that in that same year, criminals acquired a total of at least 1.2 million guns.[51] Thus, even if one unrealistically assumed that all of the 19,777 guns known to have been trafficked by ATF-investigated traffickers were sold to criminals, and if all of these were trafficked in a single year, then at most this comprised 1.6 percent of the guns acquired by criminals in that year. More realistically, if traffickers sell indiscriminately to whoever will pay, and if they therefore sold only half of their guns to criminals, then these trafficked guns would comprise less than 1 percent of the guns acquired by criminals.

There are, however, traffickers unknown to police, and there may even be high-volume traffickers who are never caught. Law enforcement evidence, the best evidence available, cannot prove a negative, such as the assertion that virtually no high-volume traffickers operate. One can only say that the law enforcement agencies charged with uncovering such trafficking have discovered few large-scale operations, have not generated affirmative evidence of widespread high-volume trafficking, and have not supplied evidence that would support an affirmative claim that traffickers supply more than a tiny share of criminals' guns.

D.   The Involvement of Licensed Dealers in Trafficking

Do corrupt or negligent FFLs contribute significantly to the flow of illicit guns to criminals? Compared to criminals who commit offenses like burglary or auto theft, illicit gun dealers should be especially easy for investigators to uncover, for the same reason that street dealers of illicit drugs are easy to identify: It must be possible for prospective customers to find the sellers. And if buyers can find them, then the police or their informants can do so as well. Licensed but corrupt dealers should be even easier to detect than unlicensed traffickers because all FFLs are known to authorities as gun dealers, required to maintain detailed records of every acquisition or disposition of a gun, and subject to close inspection of those records. Audits of these records can uncover suspicious patterns, and even if the required records are not maintained, this failure can itself serve as the basis for regulatory action, more intensive investigation, and in some cases, revocation of a dealer's license or criminal charges. Because FFL misbehavior is easier to detect, and because FFLs may

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51.   *See infra* Part I.C.

be targeted for investigation more frequently for this very reason, the FFLs' share of trafficking is likely to be overstated by law enforcement data.

Despite the relative ease of doing so, ATF discovered so little serious misconduct among FFLs that in all of fiscal year 1999 they revoked the licenses of only 20 FFLs in the entire United States—less than a fiftieth of one percent of the 103,942 total FFLs operating at that time.[52] Even when ATF selectively focused extensive compliance inspections on 1,700 dealers thought to be more likely to be involved in gun trafficking because they displayed "a range of indicators of potential firearms trafficking,"[53] few of these were found to be involved in misconduct serious enough to merit revocation of their licenses. Of the 1,700 suspect dealers inspected in 1998, ATF revoked the licenses of just thirteen, in addition to seventy-five who surrendered their licenses, were placed out of business, or were denied renewal of their licenses.[54]

Conversely, among 1,530 trafficking operations investigated by ATF during 1996–1998, only 8.7 percent involved trafficking by any FFLs.[55] Thus, few FFLs are involved in trafficking, and few trafficking operations involve FFLs. Those who believe in the importance of high-volume trafficking involving FFLs, however, stress that, on those rare occasions that an FFL is involved in trafficking, the numbers of guns trafficked are much larger than in other trafficking operations—an understandable result given an FFL's easy access to large supplies of guns.[56] Indeed, ATF figures indicate that 32 percent of guns trafficked by the operations investigated by the agency were handled by operations in which FFLs were implicated.[57] These data, however, cannot establish the share of all guns going to criminals that were moved by trafficking operations involving FFLs. ATF cautions that their investigations "do not necessarily reflect typical criminal diversions of firearms."[58] And this percentage almost certainly overstates the FFL share of trafficked guns given

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52.   U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, COMMERCE IN FIREARMS IN THE UNITED STATES—FEBRUARY 2000 A-21, A-22 (2000).

53.   *Id.* at 30.

54.   *Id.* at 31.

55.   FOLLOWING THE GUN, *supra* note 20, at 11.

56.   *See, e.g.*, *id.*; Braga et al., *supra* note 10.

57.   FOLLOWING THE GUN, *supra* note 20, at 13 tbl.3 (2000). This report indicates that 40,365 firearms were "trafficked by licensed dealer[s], including pawnbroker[s]," from among a total of 84,128 trafficked firearms identified in 114 investigations of trafficking by licensed dealers. *Id.* at 13. It is, however, inappropriate to calculate the FFL share as 40,365 out of 84,128, because ATF double-counted both its investigations and trafficked firearms in multiple "trafficking channel" categories. The sum of the firearms attributed to each separate category was 125,928, indicating that each trafficked gun was counted about 1.5 times (125,928 / 84,128 = 1.5). Using the proper base total, a more correct FFL share would be 32 percent (40,365 / 125,928 = 0.321).

58.   *Id.* at 53.

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the greater ease of detecting criminal activity within a group that Cook and Braga rightly characterize as "vulnerable to ATF's capacities for regulation and enforcement."[59]

ATF's caveat is more than merely pro forma—the agency clearly focuses disproportionately on more vulnerable investigative targets. To illustrate, 13.9 percent of ATF's 1996–1998 trafficking investigations were aimed at "gun shows and flea markets,"[60] even though the Census Bureau's 1997 Survey of State Prison inmates found that only 1.7 percent of gun criminals had obtained their crime guns from a gun show or a flea market.[61] ATF was clearly not focusing its investigations on gun show trafficking because this activity supplies a large share of crime guns. Rather, because gun shows are advertised, legal events, they may simply be easier to investigate than trafficking rings that operate secretly.

E.   The Significance of the Prices Criminals Pay for Guns

Data on prices paid for illegal guns also strongly suggest that FFL involvement in trafficking, whether knowing or negligent, is rare. Traffickers who buy guns, new or used, from FFLs at retail prices can only make a profit if they sell the guns at prices substantially higher than retail price. Further, given the need to pay straw purchasers for their services, when employed, and to cover transportation and other expenses, it is unlikely that traffickers could begin to turn a profit unless they sold guns for amounts well above—perhaps at least double—the retail price. Thus, if many criminals obtain guns through the efforts of traffickers working in this way, we should find that a large share of criminals buy guns at prices well above retail price. Interviews with criminals, however, indicate that the vast majority instead generally pay *less* than retail price for their guns. Joseph Sheley and James Wright found that 65 percent of inmates of juvenile correctional facilities and 74 percent of high school students paid less than $100 for their most recently acquired handgun,[62] at a time (about 1990) when only a handful of handguns had a retail price under $100.[63] Similarly, Wright and Rossi concluded, based on interviews with adult inmates, that even though criminals often possessed higher quality guns, they typically paid much less than retail, because "prices in the informal, gray, and black markets are heavily discounted, in all

59.   Cook & Braga, *supra* note 18, at 300.
60.   FOLLOWING THE GUN, *supra* note 20, at 11.
61.   BUREAU OF JUSTICE STATISTICS, *supra* note 38, at 6.
62.   SHELEY & WRIGHT, *supra* note 27, at 49–50.
63.   KEN WARNER, GUN DIGEST 1990 *passim* (1989).

likelihood because of the predominance of stolen weapons in these markets."[64] Thus, even though virtually all guns are sold at or near full retail price when they are new, by the time their ultimate criminal consumers acquire the guns, they generally are sold for much less. This evidence strongly suggests that traffickers were not responsible for moving the retail-priced guns from licensed dealers to criminals.

Occasional claims that criminals pay substantially above-retail prices for guns are supported only by isolated, unsubstantiated anecdotes, typically fed to uncritical reporters by ATF agents. For example, Philip Cook and his colleagues cite a newspaper article in which an ATF agent was quoted as asserting that for illegal handguns purchased in New York City there was a markup of "five times or more over the price in Virginia."[65] These authors likewise cite unsubstantiated claims by journalists that handguns purchased for $50 in Ohio were sold for $250 in Philadelphia.[66] The evidence for such journalistic claims usually turns out to be unverified anecdotes supplied by ATF agents.[67]

Some scholars even insist that criminals pay a premium over retail for illicit guns in the face of their own contradictory evidence. For example, Philip Cook and his colleagues, based on interviews with criminals in one high-crime area of Chicago, claimed at one point that there was a substantial price markup in the underground gun market.[68] Their own interviews, however, indicated that even among the more naïve, less well-connected youth in the area of their study, prices actually paid ranged from $250 to $400.[69] Assuming that the mean price paid by these youth was around the midpoint between $250 and $400, then the average price paid was $325. This is very close to the mean retail price of handguns confiscated from criminals in that same area, which was about $316.[70] This implies an average markup of just 3 percent over the average retail price, which cannot be accurately described as substantial considering that it is far less than the 15 percent markup over cost that *legal* gun retailers typically charge.[71] Thus, in a low-gun-ownership city with very

64.   WRIGHT & ROSSI, *supra* note 14 at 233.
65.   Cook et al., *supra* note 10, at 72 n.56.
66.   *Id.*
67.   *E.g.*, Richard Lacayo, *Running Guns Up the Interstate*, TIME, Feb. 6, 1989, at 24; Howard Schneider, *Gun-Control Fusillade Heats Up; Rally in Annapolis Backs Bill's Foes*, WASH. POST, Mar. 8, 1991, at C5; John F. Harris, *Gunrunning Alleged in Indictment—Trail Said to Run From VA. to N.Y.*, WASH. POST, Jan. 6, 1993, at D1.
68.   Philip J. Cook et al., *Underground Gun Markets*, 117 ECON. J. F588, F592–96 (2007).
69.   *Id.*
70.   Cook et al., *supra* note 68, at F594, F616.
71.   Cook et al., *supra* note 10, at 71 n.54.

restrictive gun laws, even more naïve young gun buyers lacking extensive criminal connections were not paying prices substantially over retail. Although prices for used guns sold by licensed retailers would not be as high as the new-gun retail prices used by Cook and his colleagues,[72] the differences in prices charged by gun dealers between new guns and near-new used guns is slight, and Cook himself has asserted that most crime guns are relatively new.[73]

Moreover, these data pertain only to an unrepresentative sample of a small segment of the population in just one unrepresentative area of Chicago.[74] Cook and his colleagues also reported considerably more statistically meaningful city-wide data on prices paid by Chicago arrestees who were interviewed in 1996–1997 as part of the U.S. Justice Department's Drug Use Forecasting program. This more systematic body of data indicated that the median price paid for handguns by Chicago criminals was just $150,[75] less than half the $331 mean new-gun retail price of the guns confiscated from Chicago criminals during that time frame.[76]

It is certainly possible that traffickers served only a segment of the criminal market covered by Cook's study, and that criminal customers in this segment do indeed pay large markups over retail. Cook and his colleagues' data, however, indicate that only 6.8 percent of Chicago arrestees paid $500 or more for their guns,[77] a price that, based on Cook's claims in 1995, should have been commonplace in areas with a relative scarcity of guns and restrictive gun laws.[78] Since some of these arrestees may have been buying guns with retail prices only modestly above $500, the share of Chicago arrestees paying markups of three or four times retail price ($900–$1200) necessarily must have been quite small.

Thus, Cook's evidence consistently contradicts his earlier claims of huge price markups, as large as four- or five-to-one,[79] and does not even support his claim that criminals pay amounts even slightly more than retail prices. Even in Chicago, where handguns have been banned since 1982 and where gun ownership was quite low even before the ban, the prices paid by criminals are generally comparable with or below retail, and thus provide no support for the theory that gun traffickers buy guns at retail prices from licensed gun

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72.   *See* Cook et al., *supra* note 68, at F616.
73.   Cook & Braga, *supra* note 18.
74.   *See* Cook et al., *supra* note 68, at F561–62.
75.   *Id.* at F573.
76.   Computed from the data provided in *id.* at F616 tbl.A4.
77.   *Id.* at F603.
78.   *See* Cook et al., *supra* note 10, at 72.
79.   *Id.* at 72 n.56.

dealers and then sell them at moderate-to-huge markups to criminals in areas with strict gun laws.

Perhaps Chicago is unrepresentative of high-control cities, and perhaps traffickers realize higher profit margins in other places with stringent controls. To provide comparative perspective, we analyzed Drug Use Forecasting data from interviews conducted in 1997 with arrestees in New York City and Washington, D.C., where handgun ownership is likewise banned. The mean price paid by arrestees for their most recently acquired handgun was $259 in New York, $219 in D.C., and $190 in Chicago.[80]

A rough estimate of the retail prices of handguns used by criminals in those cities can be obtained from published ATF data on guns recovered and submitted for tracing. The ten most frequently recovered types of guns, classified by manufacturer, caliber, and general gun type (revolver, semi automatic pistol, and so forth) are listed in ATF reports.[81] We looked up the suggested retail price of the least expensive model within each category (for example, the least expensive Ruger nine millimeter semiautomatic pistol) in the 1997 edition of *Gun Digest*, and conservatively assumed that this was the average retail price of guns in each category.[82] We weighted these prices by the number of crime guns in that category that were recovered and traced, in order to obtain an average retail price of the most popular crime guns recovered from criminals in each city. Even assuming conservatively that the least expensive handgun was used in each category, the average retail price of crime guns recovered in 1998 was $260 in New York City, $374 in Washington, D.C., and $237 in Chicago.

Thus, even in these exceptional urban areas with stringent gun controls, where traffickers are supposed to flourish, criminals pay *under* the retail price for handguns. Consequently, the notion that criminals could make significant profits by selling guns purchased at retail prices from FFLs is not plausible even in cities with unusually low gun ownership rates and unusually strict gun laws, such as New York, Washington, D.C. or Chicago. Traffickers who purchase guns at retail prices can, at best, profit only by selling to unusually ill-informed or poorly connected criminals, that is, the handful willing to pay far more than the average criminal in their city. The idea of such a trafficker profiting is even less plausible with regard to places where controls over gun sales

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80. U.S. DEP'T OF JUSTICE, NAT'L INST. OF JUSTICE, DRUG USE FORECASTING IN 24 CITIES IN THE UNITED STATES, 1987–1997 (1998) [GUN ADDENDUM DATA, 1997] (restricted version of ICPSR Study 9477 obtained from the National Archive of Crim. Just. Data, on file with author).

81. U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, *supra* note 12, at 33.

82. KEN WARNER, GUN DIGEST 1997 (1996).

are weaker, gun ownership (and thus gun theft) rates are higher, and traffickers therefore face more competition from legal dealer sales and from stolen guns.

II.   HOW DO CRIMINALS GET GUNS?

A.   The Survey Evidence

The richest sources of information on gun acquisition by criminals are surveys of incarcerated criminals.[83] The findings from direct questioning of felons are consistent with the "dispersed" model of the movement of guns to criminals, which hypothesizes that offenders most commonly steal their own guns or buy them from friends, relatives, or acquaintances. The most detailed questioning of criminals about their methods of gun acquisition was conducted by James Wright and Peter Rossi, who found that theft was an especially important method.[84] When asked how they had obtained their most recently acquired handgun, 32 percent of felons reported that they personally stole the gun. The prisoners were also asked if they believed that their most recently acquired handgun was stolen, and 46 percent stated that the weapon was "definitely stolen" (these inmates presumably included the 32 percent who reported having personally stolen the gun). Another 24 percent indicated the weapon was "probably stolen."[85] Thus, the criminals believed that 46–70 percent of their handguns were stolen.

This study also found that criminals do not typically seek out guns to steal, but rather steal those they happen to come across in the course of criminal activity,[86] most commonly thefts from homes or vehicles.[87] Criminals usually sell the guns they steal, but most gun thieves have also retained at least one gun for their own use. They typically kept the gun because the stolen weapon was a "nice piece," rather than because they did not already have one.[88] Thus, the criminals evidently used theft as a way of upgrading the quality of their weaponry, rather than as a way of becoming armed. Surveys also indicate that

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83.   *See, e.g.*, SHELEY & WRIGHT, *supra* note 27; U.S. BUREAU OF JUSTICE STATISTICS, SURVEY OF STATE PRISON INMATES 1991 (1993); U.S. BUREAU OF JUSTICE STATISTICS, *supra* note 38; WRIGHT & ROSSI, *supra* note 14; *cf.* DAVID C. MAY & G. ROGER JARJOURA, ILLEGAL GUNS IN THE WRONG HANDS 8–9 (2006).

84.   WRIGHT & ROSSI, *supra* note 14, at 198–204.

85.   *Id.* at 196.

86.   *Id.* at 200.

87.   *Id.* at 206.

88.   *Id.* at 201–02.

offenders believe that they can get guns from multiple types of sources; therefore, eliminating a single channel would likely not prevent the acquisition of a gun.[89]

Wright and Rossi also found that 16 percent of the felons' handguns had been purchased from retail (presumably licensed) sources,[90] although their questions did not differentiate between a felon buying the gun directly and a felon using a straw purchaser. The authors did not ask whether the felon had any disqualifying criminal convictions at the time of the purchase, so it is impossible to tell whether any of these guns were acquired unlawfully, were straw-purchased, or involved unlawful behavior or negligence on the part of the retail seller.[91] Nevertheless, even some scholars who have adopted the theory that traffickers use straw purchasers to acquire guns from FFLs concede that criminals rarely use straw purchases from FFLs to obtain guns for themselves.[92]

Although the surveys provide little direct support for the concentrated flow model or the organized trafficking model, this at least partly reflects the limits of the method. Criminals typically know only the proximate source of their guns—the person from whom they directly obtained a gun. They usually would not know whether traffickers were involved in earlier movements of the gun, further back in the chain of possession. A buyer also would not always know whether the proximate source was regularly engaged in illicit gun sales. In any case, the questions asked in past studies have not been framed in a way that allows researchers to distinguish sources who regularly and persistently sold illicit guns from those who did so on only on a few occasions. Thus, while the survey evidence does not support the view that traffickers channel a significant share of the guns obtained by criminals, neither does it rule it out.

B.   Evidence from Traced Crime Guns

The belief in the importance of persistent, organized, or high-volume gun trafficking is largely based on indirect inferences from information on guns that are seized or recovered from apprehended criminals and then traced by ATF. The process of tracing a gun works as follows: When a criminal is arrested and found to possess a gun, or when a gun is otherwise recovered by police

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89.   *Id.* at 210–15; SHELEY & WRIGHT, *supra* note 27, at 47; MAY & JARJOURA, *supra* note 83, at 37, 47 tbl.3.1.

90.   WRIGHT & ROSSI, *supra* note 14, at 185.

91.   *Id.*

92.   Cook et al., *supra* note 68, at F566–F567; Daniel W. Webster et al., *How Delinquent Youths Acquire Guns*, 79 J. URB. HEALTH 60, 65–66 (2002).

and it is known or suspected to be a crime gun, law enforcement officers may submit a request to ATF for that gun to be traced. This means that its history is established, as officially recorded on various legal forms, hopefully up to the point of first retail sale—when it was first sold as a new gun. ATF typically does this by first contacting the manufacturer or importer (or, equivalently, by consulting a manufacturer's computer database supplied to ATF) in order to identify the distributor (wholesaler) to whom the gun was sold by the manufacturer or importer. ATF then contacts this distributor to establish the identity of the licensed retail dealer to whom the gun was sold. Finally, ATF contacts the retail dealer who sold the gun, in order to establish who first purchased the new gun. If all necessary records were completed and remain available, the gun can be traced as far back as its first private owner, at which point the paper trail ends, since ATF typically does not have access to records of transfers (including thefts) that occur after the first retail sale.[93] A criminal who uses a gun to commit a violent crime is rarely the weapon's first retail purchaser, so tracing alone rarely identifies a previously unknown suspect. Indeed, most crime guns become available for tracing only because they were recovered from criminal possessors at the time of their arrest. ATF and local law enforcement agencies more commonly use trace data for the purpose of identifying unlicensed traffickers or high-risk potentially corrupt FFLs.[94]

C.   Putative Gun-Trafficking Indicators

ATF has identified a number of indicators that it believes are correlated with a heightened probability that a given crime gun was trafficked.[95] If indicator data are aggregated up to the dealer level, high risk dealers may be identified. In other words, FFLs who sell many guns with these traits, or who have many crime guns traced back to them, may be engaged in criminal or irresponsible gun selling. Further, if the data on crime guns are aggregated up to the city level, some of these indicators may also be useful measures of the relative prevalence of gun trafficking among cities.

ATF has not directly validated any of these indicators, for example, by demonstrating that it can efficiently differentiate trafficked guns from nontrafficked guns, or that it can identify dealers who were later found, through law enforcement investigation or inspection of dealer records, to be traffickers. Nor has ATF made any specific claims as to what share of trafficked

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93.   U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, *supra* note 40, at 68 (2002).
94.   *Id.* at 64.
95.   *E.g.*, *id.* at ix.

guns or corrupt dealers are characterized by any given indicator. Scholars who use ATF's indicators have generally simply assumed their validity, based largely on ATF arguments as to why they should be associated with trafficking.[96]

An effective indicator of trafficking would have two attributes: (1) it would be substantially more common among trafficked guns than among nontrafficked guns, and (2) a large share of guns with this trait would be trafficked guns. If a potential indicator possessed the first attribute but not the second, it would be an inefficient tool for identifying trafficked guns, since a large share of guns characterized by the indicator would be false positives. In other words, they would be predicted to be trafficked guns when they were not. For example, suppose that 5 percent of guns possessing trait X were trafficked, while only 1 percent of guns without trait X were trafficked. Guns with the indicator are then five times more likely to have been trafficked than guns without the indicator, yet trait X would still have little value for identifying trafficked guns, because 95 percent of guns with trait X were not trafficked. It would be wasteful to direct investigative resources at FFLs who sold guns with this trait. Thus, the absolute prevalence of trafficking among guns with a given indicator is essential in assessing the indicator's utility. Nonetheless, ATF makes no claims about the approximate share of guns with any of its preferred indicators that it believes were trafficked, or about the share of trafficked guns characterized by a given indicator. For example, ATF has never asserted that even as much as 10 percent of crime guns recovered by police within three years of first retail sale (sometimes loosely described as "new" guns) were trafficked. Nor, conversely, has ATF asserted that at least 10 percent of trafficked guns are recovered within three years.

The Brady Center to Prevent Gun Violence is among those entities who have misunderstood this limitation, claiming that ATF believes that crime guns with a "time-to-crime" (which is more accurately described as "time-to-recovery," or TTR) of under three years "*likely* were trafficked out of licensed dealers into the criminal market."[97] That is, the Brady Center asserted that ATF believes that most new crime guns were trafficked. However, ATF merely states, in its characteristically ambiguous way, "To the investigator, the short time from retail sale to crime, known as 'time-to-crime,' *suggests* illegal diversion or criminal intent associated with the retail purchase from

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96.   *See, e.g.*, Cook et al., *supra* note 10; Pierce et al., *supra* note 10; Daniel W. Webster et al., *Effects of a Gun Dealer's Change in Sales Practices on the Supply of Guns to Criminals*, 83 J. URB. HEALTH 778 (2006).

97.   BRADY CTR. TO PREVENT GUN VIOLENCE, WITHOUT A TRACE: HOW THE GUN LOBBY AND THE GOVERNMENT SUPPRESS THE TRUTH ABOUT GUNS AND CRIME 11 (2006), *available at* http://www.bradycenter.org/xshare/pdf/reports/giw.pdf (emphasis added).

the FFL."[98] ATF thus does not claim that even 1 percent of new crime guns were trafficked, much less a majority or even many of them.

The most common logical fallacy that appears to underlie misinterpretation of tracing-based indicators is that of "affirming the consequent."[99] An analyst accurately notes that a large share of trafficked guns possesses attribute X, but then draws conclusions that follow only if the converse was true—if a gun has attribute X, it is certain or likely that it has been trafficked. Perhaps the most extreme example of this misinterpretation was by Daniel Webster, Jon Vernick, and Maria Bulzacchelli, who labeled all guns with a time-to-crime of under one year, and whose criminal possessor was not the original retail purchaser, as "new trafficked crime guns."[100] In fact, virtually all of these guns may simply have been stolen from their lawful buyers within a year of purchase.

In other research, this logical fallacy is implicit rather than overt. Glenn Pierce and his colleagues carried out a long series of statistical analyses exploring what traits of crime guns were associated with a short TTR.[101] Their key underlying assumption was that a short TTR is an indicator of trafficking or illegal diversion of guns. The authors inferred that other traits that were correlated with short TTR were also indicators that the gun had been trafficked. They did not explicitly assert that all or even most guns with a short TTR are trafficked or illegally diverted, but instead merely repeated the vague ATF claim that guns with this trait, in combination with other indicator traits, "may have been illegally diverted from legal commerce."[102] Obviously one can always infer that any given crime gun *may* have been trafficked, even without making use of any supposed trafficking indicators. This weak assertion leaves open the possibility that nearly all guns with a short TTR are *not* trafficked guns, in which case most or nearly all variation in TTR across crime guns is likely to be unrelated to whether the guns were trafficked. Consequently, any associations discovered between short TTR (or any other weak indicator) and other variables may tell us nothing about the correlates of trafficking history. The conclusions drawn by Pierce and his colleagues therefore embody the fallacy of affirming the consequent, by assuming that a large share of guns with short TTRs had been trafficked—an assumption with no empirical support.

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98.   U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, *supra* note 40, at ix (emphasis added).
99.   This fallacy is committed if one starts with the premise: If P, then Q. Upon observing that Q is true, one then (wrongly) concludes: Therefore, P is true.
100.   Webster et al., *supra* note 96, at 779.
101.   Pierce et al., *supra* note 10, 391–422.
102.   *Id.* at 402.

We consider below the most commonly discussed trafficking indicators, including dealer-level traits of FFLs that may point to their involvement in trafficking (for example, a large number of crime guns being traced back to a dealer). We do not consider measures of the thoroughness or effectiveness of ATF enforcement actions, such as number of compliance inspections conducted, because the corresponding data are not available for use at the city level.

1.   Shorter Time-to-Recovery (TTR)

Like legitimate businesses, gun traffickers likely seek to make sales quickly and avoid accumulating large unsold inventories, so they work to move their guns quickly from first retail sale (in which the trafficker or a straw-purchaser associate buys a gun) to a sale by the trafficker to his customer. The more quickly this happens, the sooner a gun is likely to end up in a criminal's possession, be used in a crime, recovered by police (usually in connection with the criminal possessor's arrest), and traced. Thus, ATF has long regarded a short TTR as an indicator that a gun has been trafficked.[103] However, firearms stolen by thieves who steal (and sell) a few relatively new guns each year are also likely to have a short TTR. Anyone who wants to profit from an illicit sale would prefer to do it quickly, and thieves also want to minimize the time they are in possession of stolen property. As will be explained, newer guns are disproportionately likelier to be stolen, and then purchased by other criminals. Thus, like trafficked guns, newer stolen guns will move quickly into the hands of criminals, and a short TTR does not imply anything about how a gun came into a criminal's possession.

Many guns move quickly into criminal hands because they were stolen from their owners shortly after retail purchase. A short average TTR among traced crime guns in a given area therefore may serve more as an indirect indicator of rates of property crime, especially burglary, in that area than of widespread firearms trafficking. Anthony Braga and Glenn Pierce reported data on the percent of recovered handguns in Boston that had a TTR less than three years, for the period 1996–2003, and interpreted declines in this percentage as evidence of declining gun trafficking in Boston.[104] We computed the cross-temporal Pearson's correlation between their figures for the percent of crime guns with TTRs under three years and Boston's burglary

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103.   CONCENTRATED URBAN ENFORCEMENT, *supra* note 20; U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, *supra* note 40; Zimring, *supra* note 10.
104.   Braga & Pierce, *supra* note 10, at 740–42.

rate, as reported in the Uniform Crime Reports (1997–2004),[105] and found it to be an extremely strong 0.89. The higher an area's crime rate, the shorter the time before the next crime occurs and thus the sooner any given firearm will be stolen from its lawful owner and used to commit a crime. In the absence of any direct evidence of a correlation between TTR trends and actual trafficking rates, it appears to be more likely that short-TTR guns are the result of thefts of relatively new guns than the result of high-volume, FFL-involved trafficking. Thus, it is likely that the share of a city's crime guns with short TTRs serves as an indirect indicator of the gun theft rate in that city.

Consequently, licensed dealers whose traced guns have shorter TTRs cannot be assumed to be involved in trafficking. Shorter TTRs would characterize guns sold by dealers located in or near high-crime neighborhoods, regardless of whether the dealers were operating in an unlawful or irresponsible fashion. One would likewise expect a shorter average TTR among those models or types of guns, such as inexpensive handguns, that are especially popular as self-defense weapons in high-crime areas, since they would be more likely to be stolen.

Gun thieves, of course, steal older guns as well as new ones, but are more likely to retain the better ones (presumably the newer ones) for their own use.[106] Criminals presumably prefer newer guns to old ones, just as criminals and noncriminals alike generally prefer new varieties of almost any consumer good to older ones. Among noncriminals, new guns would, on average, cost more to buy than their used counterparts, but among criminals who obtain their guns by theft, a preference for new guns costs nothing to indulge. For this reason alone one would expect a larger share of guns to be new among criminals than among noncriminals. Criminals who steal guns are presumably

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105.   *See* FEDERAL BUREAU OF INVESTIGATION, U.S. DEP'T OF JUSTICE, CRIME IN THE UNITED STATES 2004, at 100 (2005); FEDERAL BUREAU OF INVESTIGATION, U.S. DEP'T OF JUSTICE, CRIME IN THE UNITED STATES 2003, at 96 (2004); FEDERAL BUREAU OF INVESTIGATION, U.S. DEP'T OF JUSTICE, CRIME IN THE UNITED STATES 2002, at 92 (2003); FEDERAL BUREAU OF INVESTIGATION, U.S. DEP'T OF JUSTICE, CRIME IN THE UNITED STATES 2001, at 90 (2002); FEDERAL BUREAU OF INVESTIGATION, U.S. DEP'T OF JUSTICE, CRIME IN THE UNITED STATES 2000, at 88 (2001) [hereinafter FBI 2000]; FEDERAL BUREAU OF INVESTIGATION, U.S. DEP'T OF JUSTICE, CRIME IN THE UNITED STATES 1999, at 85 (2000) [hereinafter FBI 1999]; FEDERAL BUREAU OF INVESTIGATION, U.S. DEP'T OF JUSTICE, CRIME IN THE UNITED STATES 1998, at 85 (1999) [hereinafter FBI 1998]; FEDERAL BUREAU OF INVESTIGATION, U.S. DEP'T OF JUSTICE, CRIME IN THE UNITED STATES 1997, at 90 (1998) [hereinafter FBI 1997]. Burglaries per 100,000 population for 1996–2003 were (in chronological order): 914, 775, 645, 612, 710, 713, 642, and 737 (computed from City of Boston crime counts and population estimates), while the percent of crime guns with a TTR under three years was 53.8, 36.6, 24.9, 15.6, 15.1, 19.3, 15.5, and 22.3 (derived from Braga and Pierce, *supra* note 10, at 740).

106.   *See* WRIGHT & ROSSI, *supra* note 14, at 200–01 (noting that 68 percent of gun thieves who kept a stolen gun for personal use did so because it was "nicer" than the one they were currently carrying).

likely to retain, and later use in crimes, the newer guns. Among those stolen guns sold by the thief, the newer ones are also likely to be the most attractive to the gun thief's customers, and the first sold, other things being equal. This would help to explain why guns with a short TTR comprise a disproportionately large share of recovered crime guns.

In addition, biases in samples of guns submitted for tracing are likely to exaggerate the share of short-TTR guns. Because newer guns are likely to have changed hands fewer times between retail sale and recovery in a crime, they have more value for the investigation of gun trafficking, since it is more likely that authorities can link such a crime gun to a trafficker or to a corrupt licensee. Consequently, police are likely to prefer to submit trace requests on newer guns, which would result in short-TTR guns claiming a larger share of traced crime guns than of all recovered guns.

Pierce and his colleagues disputed the idea that a large share of crime guns had been stolen, reasoning that "if most crime guns were stolen or were sold . . . as part of legal private transactions, we would expect to have an age distribution of crime guns that closely resembles the age distribution of firearms produced for sale in [the] United States."[107] They found that traced guns do not show such an age distribution, and concluded that most crime guns had not been stolen or sold in legal private transfers. However, this age distribution of traced guns is partly an artifact of the biased nature of traced-gun samples—they over-represent newer guns. But even ignoring this problem, the authors' reasoning is itself fallacious, because it implicitly assumes that, unlike virtually everyone else, criminals have no preference for newer guns, and in effect randomly choose, from among the available pool of stolen weapons, the guns they keep for themselves and later use in crime. Thus, the fact that newer guns are disproportionately involved in crime is not at all inconsistent with the proposition that most crime guns are obtained directly or indirectly by theft. Rather, the age distribution of crime guns suggests that, even though most of the firearms obtained by criminals may have been stolen, and many of these stolen weapons were older guns, gun thieves and other criminals prefer to retain, and use in crimes, the newer weapons.

There are still other reasons why one would expect relatively new guns to comprise a large share of crime guns, even if few were purchased by traffickers and quickly sold to criminals. First, crime victims are disproportionately

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107.   GLENN A. PIERCE ET AL., THE CHARACTERISTICS AND DYNAMICS OF CRIME GUN MARKETS: IMPLICATIONS FOR SUPPLY-SIDE FOCUSED ENFORCEMENT STRATEGIES 38 (2003), *available at* http://www.ncjrs.gov/pdffiles1/nij/grants/208079.pdf.

young,[108] and the property owned by younger people tends to be relatively new. For example, among a randomly selected sample of 339 handguns reported in the 1994 National Survey of the Private Ownership of Firearms, the mean number of years that 18–24-year-old respondents had owned the gun was 2.7 years, compared to 4.8 years among those aged 25–39, 11.8 years among those aged 40–64, and 20.7 years among those aged 65 or older.[109] Thus, the higher rate of victimization among younger people implies that newer guns have a greater chance of being stolen, and thereby comprise a disproportionately large share of the guns possessed by criminals. Further, crime guns that were directly and lawfully purchased from FFLs by criminal users will be disproportionately new when used in crimes simply because criminals are themselves disproportionately young and thus likely to have been gun owners for shorter periods of time.

At the city level, if one interpreted the prevalence of guns with a short TTR among recovered crime guns as an indicator of the involvement of gun traffickers in supplying guns to criminals, one would be forced to draw some very dubious conclusions about where gun trafficking is most common. The consensus among scholars is that organized or systematic illicit trade in guns will be more profitable and thus more common in places where the acquisition of guns is more strictly regulated and gun ownership levels are lower.[110] Table 1 shows that all of the cities where gun trafficking is thought to be commonplace—due to strict local gun laws and low noncriminal gun ownership levels—actually have *longer*-than-average TTRs than other cities. In New York, Boston, and Chicago, three cities with some of the strictest controls in the nation, crime guns on average actually take longer to reach criminals' hands than crime guns in other cities. Therefore, if one views shorter-than-average TTR as an indicator of the prevalence of gun trafficking, one would have to conclude that there is less gun trafficking taking place in these cities with relatively strict gun controls. Conversely, crime guns recovered in many cities with higher gun ownership rates, weaker gun laws, and thus little need for the services of gun traffickers, have very short average TTRs. Such cities include Albuquerque, Atlanta, Greensboro, Memphis, Nashville, New Orleans, Phoenix, Richmond, and Tucson. This observed pattern makes

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108.   *See* BUREAU OF JUSTICE STATISTICS, U.S. DEP'T OF JUSTICE, CRIMINAL VICTIMIZATION IN UNITED STATES, 1994 STATISTICAL TABLES, tbl.84.

109.   *See* POLICE FOUND., NATIONAL STUDY OF PRIVATE OWNERSHIP OF FIREARMS IN THE UNITED STATES, 1994, ICPSR version, (1998), *available at* http://www.icpsr.umich.edu/access/ index.html.

110.   *See, e.g.*, Cook & Braga, *supra* note 18, at 308; Braga et al., *supra* note 10, at 333; Cook et al., *supra* note 10, at 72; D.W. Webster et al., *Relationship Between Licensing, Registration, and Other Gun Sales Laws and the Source State of Crime Guns*, 7 INJ. PREVENTION 184 (2001).

sense if a shorter average TTR mostly reflects high rates of gun theft, and if crime guns that move quickly into criminal hands are more prevalent in cities with high rates of gun ownership and high rates of gun theft. We empirically test this hypothesis later.

TABLE 1. DOES A SHORT AVERAGE TIME-TO-RECOVERY (TTR) INDICATE A HIGH LEVEL OF GUN TRAFFICKING?[111]

City	% Traced Guns with TTR < 3 years	Median TTR (in years)
Albuquerque, NM	43	4.7
Anaheim/Long Beach, CA	14	8.8
Atlanta, GA	49	3.1
Austin, TX	33	6.2
Baltimore, MD	26	6.8
Baton Rouge, LA	43	6.1
Birmingham, AL	29	3.0
Boston, MA	19	7.9
Buffalo, NY	30	6.6
Camden, NJ	27	6.1
Charlotte-Mecklenburg, NC	41	4.4
Chicago, IL	29	6.2
Cincinnati, OH	38	5.4
Cleveland, OH	33	6.5
Dallas, TX	29	6.6
Denver-Aurora, CO	38	4.9
Detroit, MI	26	6.9
Gary, IN	53	2.6
Greensboro, NC	39	4.6
Houston, TX	26	7.1
Indianapolis, IN	49	3.1
Jacksonville, FL	24	6.7
Jersey City, NJ	31	6.4
Las Vegas, NV	39	4.5

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111.   U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, *supra* note 40 (drawing figures from each corresponding city report pertaining to 2000).

Los Angeles, CA	17	8.0
Louisville, KY	38	5.5
Memphis, TN	35	5.1
Miami, FL	28	6.5
Milwaukee, WI	41	4.6
Minneapolis, MN	34	5.3
Nashville, TN	33	5.4
New Orleans, LA	39	5.0
New York City	21	7.4
Newark, NJ	28	6.5
Oakland, CA	19	8.0
Oklahoma City, OK	25	6.5
Philadelphia, PA	44	3.8
Phoenix, AZ	35	5.1
Pittsburgh, PA	16	7.8
Portland, OR	30	6.0
Richmond, VA	38	4.6
Salinas, CA	24	6.7
San Jose, CA	19	9.0
San Antonio, TX	26	6.9
Seattle, WA	46	4.1
St. Louis, MO	18	7.6
Stockton, CA	17	9.2
Tampa, FL	25	6.7
Tucson, AZ	43	4.0
Washington, D.C.	31	5.7
U.S.	31	6.1

In sum, though trafficked guns are likely to have a short TTR, this does not imply that guns with a short TTR are likely to have been trafficked. New York City (NYC) is commonly regarded as a place where gun traffickers are especially important as suppliers of criminals' guns, since there are virtually no sales of handguns to the general public by licensed dealers within the city.[112] If the ATF's view of TTR were accurate, one would expect to find that a large share of NYC crime guns move quickly from retail sale to recovery by NYC law enforcement. In fact, among NYC guns traced in 2000, only 11 percent had

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112.   VIZZARD, *supra* note 28, at 31.

a TTR under one year,[113] even lower than the comparable 15-percent share that prevailed in nationwide.[114] That is, looking only at TTR, only about a tenth of the city's traced guns moved quickly enough into criminals' possession to look like trafficked guns. Even fewer crime guns possessed multiple indicators.

2.   Out-of-State (OOS) Origins

Some traffickers or their straws buy significant numbers of guns in batches from sources in states with weaker gun control laws, and then sell the guns in high-control states.[115] A significant volume of interstate gun smuggling would suggest that substantial numbers of crime guns were first purchased in a state different from the one in which police recovered them. It certainly is true that many guns used in crimes had previously been moved across state lines. Some scholars, however, have overinterpreted this fact as signaling something about the prevalence of interstate gun smuggling. For example, Jeremy Travis and William Smarrito asserted that guns were being supplied to NYC criminals by "a highly effective interstate black market," based almost entirely on the fact that a large share of those guns were originally purchased in a different state.[116] An out-of-state (OOS) origin, however, is not necessarily an indicator of the involvement of gun-smuggling traffickers, since there are mundane alternative explanations for cross-state movement, such as the gun being moved by its owner upon a change of residence and then being stolen.

NYC provides a useful extreme case study, since an unusually large share of its crime guns have OOS origins—84.5 percent of those traced in 2000, compared to 38 percent of guns recovered nationwide.[117] Given that virtually no private citizen may legally buy handguns in NYC, it is scarcely surprising that few crime handguns were first purchased in NYC. Does interstate gun smuggling into NYC, however, account for this cross-state movement of guns, or could routine migration of gun owners produce the same result? Census Bureau data indicates that in 2000, 798,565 of NYC's residents had been born in a different state, 368,388 of them in the South. All of these NYC residents necessarily lived in a different state, and then moved to New York. Still other residents were born in New York, moved to another state, and then moved

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113.   U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, *supra* note 40, New York Section, at 5.

114.   *Id.* at ix.

115.   BRADY CTR. TO PREVENT GUN VIOLENCE, *supra* note 97, at 14.

116.   Jeremy Travis & William Smarrito, *A Modest Proposal to End Gun Running in America*, 19 FORDHAM URB. L.J. 795, 802 (1992).

117.   *See* U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, *supra* note 40, at 16 tbl.F (noting that only 15.5 percent of traced crime guns recovered in New York City were originally sold within the state of New York).

back to New York. In just the five-year period between 1995 and 2000, 301,243 people moved from a different state to NYC.[118] These migrants presumably moved their possessions with them. If handgun ownership among these migrants was equal to U.S. average (at least 0.325 handguns per person),[119] migrants born in other states would have moved about 260,000 handguns from other states into NYC, and recent migrants alone would have moved around 98,000 handguns just in the preceding five-year period, about 20,000 per year. At this rate, over a period of a single seventy-year human life span, 1.4 million OOS handguns would have been moved into the city, lending some credence to the admittedly extreme guess by the Intelligence Division of the New York Police Department that there were two million illegal handguns in the city in 1980.[120] While some migrants who are both law-abiding and aware of New York's strict gun laws no doubt leave their handguns behind, others surely do not, either due to ignorance, or due to a judgment that retaining their handguns is more important than obeying gun laws. Among migrants, criminals would be especially likely to move their handguns with them, both because they are more willing to violate gun laws, and because they expect to need them for criminal activity and for self-protection.

As a standard of comparison, in 2003 a total of 3,666 violent crimes (homicides, robberies, and assaults) known to the police were committed with guns in NYC.[121] Even if one implausibly assumed that each gun crime involved a different gun, thereby maximizing the number of crime-involved guns, the criminal population needed at most 3,666 guns to commit all of the known violent gun crimes in NYC.

These numbers do not suggest either that all of NYC's crime handguns actually do arrive through people moving to the city, or that 1.4 million handguns have actually arrived in the city in this way over the course of the past seventy years. But these numbers do establish that all handguns used in crime in a given year easily could have been arrived in this way, without any organized gun smuggling. Thus, routine cross-state migration of gun owners provides a credible alternative explanation for cross-state movement of the city's crime guns. Further, still other mechanisms besides interstate gun-running

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118.   U.S. CENSUS BUREAU, TABLE 2, NET MIGRATION FOR THE POPULATION 5 YEARS AND OVER FOR THE UNITED STATES, REGIONS, STATES, COUNTIES, NEW ENGLAND MINOR CIVIL DIVISIONS, AND METROPOLITAN AREAS: 2000, CENSUS 2000, *available at* http://www.census.gov/ population/www/cen2000/briefs/phc-t22/tables/tab02.pdf (county-level data from New York section of Table 2).

119.   GARY KLECK, TARGETING GUNS: FIREARMS AND THEIR CONTROL 97 tbl.3.1 (1997).

120.   *Illegal Guns By the Millions Filling City*, N.Y. TIMES, Mar. 31, 1980, at B1.

121.   Memorandum From Joe Pascarella to Commanding Officer, Office of Management Analysis and Planning, Police Department, City of New York (Mar. 18, 2005).

move guns across state lines. Any NYC resident can get a handgun if she or he has a friend or relative in another state who is willing to buy a handgun for them. A one-time straw purchase of this sort would be unlawful, but it would be misleading to label either participant a trafficker.

After arrival in the city, many guns will inevitably move into criminal possession through residential burglary, vehicle theft, and other thefts. The last large-scale victimization survey conducted in NYC estimated that there were 184,100 household burglaries in 1972,[122] at a time when the city had about 2,832,036 occupied housing units.[123] Thus, assuming no repeat victimization within a year, an average NYC residence had a 6.5 percent chance of being burglarized. Homes in high-crime neighborhoods, where handgun possession for self-protection may be higher, had a still higher risk of burglary. At this rate, a home containing a handgun would have about a 49 percent chance of being burglarized within a decade.[124]

To be sure, gun smuggling does move at least a few handguns into NYC, given that law enforcement agencies occasionally uncover gun smuggling operations, albeit typically small-scale ones. There are evidently a few criminals who do not appreciate the difficulties of making a living from gun-running, particularly the risks associated with contacting large numbers of paying customers without coming to the attention of police. And the frequent news stories of guns being purchased "down South" for $100 and sold "on the streets" of NYC for $600[125] may inadvertently encourage occasional attempts at high-volume gun-running by especially naïve criminals. Nevertheless, as previously noted, over the period from 1990 to 2006, only six trafficking operations that moved a hundred or more guns were reported in NYC newspapers—about one every three years. There is no evidence that the total

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122.   U.S. DEP'T OF JUSTICE, CRIMINAL VICTIMIZATION SURVEYS IN THE NATION'S FIVE LARGEST CITIES: NATIONAL CRIME PANEL SURVEYS OF CHICAGO, DETROIT, LOS ANGELES, NEW YORK, AND PHILADELPHIA 44 (1975).

123.   Interpolated from 1970 and 1980 Census data, in BUREAU OF THE CENSUS, U.S. DEP'T OF COMMERCE, COUNTY AND CITY DATA BOOK, 1977 at 723; BUREAU OF THE CENSUS, U.S. DEP'T OF COMMERCE, COUNTY AND CITY DATA BOOK, 1983 at 753.

124. $1-(1-0.065)10=0.49$ (The probability of any one NYC household suffering a burglary over a ten year period would be one minus the probability of not being burglarized over that period. The probability of not being burglarized in any of the ten years would equal the probability of not being burglarized in any one year, raised to the tenth power, i.e. multiplied times itself ten times. The probability of burglary in any one year was 0.065, so the probability of not experiencing a burglary in any one year was $1-.065$ or 0.935, and the probability of not being burglarized in any of ten years would be 0.935 raised to the tenth power, or 0.51. Thus, the probability of being burglarized at least once over the ten year period would be $1-0.51=0.49$, or 49 percent).

125.   *See, e.g.*, Patrice O'Shaughnessy, *Students Major in Running Guns*, N.Y. DAILY NEWS, Sept. 29, 2002, at 4, *available at* https://www.nydailynews.com/archives/news/2002/09/29/2002-09-29_students_major_in_running_gu.html.

number of guns trafficked into the nation's largest city in a typical year is more than a few hundred—a tiny number compared to the 20,000 or so handguns that could move into the city annually as a byproduct of the routine migration of gun owners.

If ordinary migration followed by gun theft, rather than gun smuggling, accounts for the vast majority of cross-state movement of crime guns, one would expect that crime guns with OOS origins would be especially likely to originate in states with high gun ownership rates, since a higher share of migrants from such states would own guns in the first place. ATF trace data indicate that this is indeed the observed pattern. For example, among NYC crime guns recovered in 2000, the leading source states were New York (15.5 percent), Virginia (14.0 percent), North Carolina (9.4 percent), and Georgia (9.2 percent).[126] Based on 2001 state-level surveys, all of the three leading originating states had rates of household gun ownership higher than the national average.[127] While some scholars have interpreted such patterns as indicating that OOS crime guns tend to originate in places with weaker gun laws,[128] there is no evidence that weakness of gun laws in source states has any impact on the patterns of interstate movement of guns, independent of the higher gun-ownership levels that tend to prevail in those same states.

3.   Criminal Possessor Was Not the Gun's First Retail Purchaser

If a trafficker was involved in moving a gun into the possession of another criminal, it follows that the criminal found by police to possess the gun is different from the person recorded on the initial purchase form (ATF Form 4473). This logic, however, cannot be reversed; it cannot be assumed that a large share of crime guns found in the possession of a person other than the first purchaser are trafficked guns. There are an enormous number of private transfers of used guns among noncriminal Americans. A national survey in 1994 found that 36 percent of guns and 31 percent of handguns acquired by the general public were acquired used.[129] Likewise, anytime a thief steals a gun and sells it to another criminal there is an intermediate possessor (the thief) even if no trafficker ever possessed the gun. Because it is so commonplace

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126.   U.S. BUREAU OF ALCOHOL, TOBACCO AND FIREARMS, *supra* note 40, at 16 tbl.F.

127.   *See* Catherine A. Okoro et al., *Prevalence of Household Firearms and Firearms-Storage Practices in the 50 States and the District of Columbia*, 116 PEDIATRICS e370, e372 (2005).

128.   *E.g.*, Braga et al., *supra* note 10, at 333 (stating that many crime guns recovered in cities with tight firearm controls originated in southern states with less restrictive controls); Pierce et al., *supra* note 10, at 401 (stating that because New York and Boston have relatively strict gun controls, "a higher percentage of guns are imported into these cities from dealers in states with weaker controls").

129.   *See* COOK & LUDWIG, *supra* note 34, at 25 tbl.3.11.

that nontrafficked guns come to be possessed by people other than the first retail purchaser, this trait is likely to be at best a weak indicator that a gun was trafficked. It may also be an indirect indicator of out-of-state origins, if one accepts the premise that the further an object travels, the more likely it is that it was possessed by more than one person.

4.   Guns Part of a Multiple-Handgun Sale

Based on the theory that traffickers acquire substantial numbers of guns by buying them in relatively large batches from corrupt or negligent licensed dealers, ATF equivocally states that "the acquisition of handguns in multiple[-handgun] sales *can* be an important trafficking indicator."[130] Philip Cook and Jens Ludwig even interpret trace data as indicating that handguns sold as part of a multiple-handgun sale (MHS) "are much more likely than others to move quickly into criminal use."[131] However, more recent evidence indicates that this conclusion is wrong; it is not true that a large share of MHS guns are trafficked, or that MHS handguns are more likely to end up in criminal hands.[132] If the typical MHS involved the purchase of dozens or hundreds of handguns, it would be reasonable to regard a MHS as highly suspect. But if MHS transfers more commonly involve just two or three handguns, this inference is weak. In fact, lawful concurrent purchases of small numbers of handguns are quite common. To illustrate, Christopher Koper found that 27 percent of all handguns sold by licensed dealers (not just those later used in crimes) in Maryland in 1990–1995 were sold as part of a MHS.[133]

Likewise, few MHS guns show signs of having been trafficked. As will be discussed later, there is good reason to view an obliterated serial number (OSN) as the strongest indicator that a gun has been trafficked. Yet, hardly any traced crime handguns that were originally sold in multiples have an OSN. Even when ATF examined a sample of handguns biased to over-represent handguns with OSNs (by analyzing only handguns from eight cities that requested traces on large numbers of guns with OSNs), it found that only 2.2 percent of MHS handguns had an OSN.[134]

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130.   U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, *supra* note 40, at ix (emphasis added).

131.   Braga, *supra* note 18, at 300.

132.   Koper, *supra* note 10, at 760.

133.   *Id.* at 758.

134.   U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, *supra* note 40, at 52. The OSN data came from just the eight cities (of forty-six total cities contributing to the 2000 national tracing report) that requested traces from ATF on at least eighty-five crime guns with OSNs. *Id.* at 50.

Further, it does not appear to be true that MHS guns are more likely to be used in crimes. Koper studied guns sold in Maryland and found that handguns sold as part of a MHS were slightly less likely to end up being used in a crime than those sold separately from other handguns. Even ten years after initial sale, only 4.1 percent of MHS handguns had been recovered by police in connection with a crime—slightly less than the 4.7 percent of single-purchase handguns linked with crimes.[135] This pattern directly contradicts the claim that MHS handguns are more likely than other handguns to be trafficked and later used in crime. Even though some traffickers do buy guns in multiples, very few guns sold in multiples show signs of being trafficked.[136] Likewise, a dealer-level study by Garen Wintemute and his colleagues found no significant relationship between a dealer's volume of MHS transactions and the rate at which crime guns were traced to the dealer.[137] The fact that a handgun was sold as part of a MHS is consequently unlikely to have much utility for identifying trafficked guns, and it is unlikely that geographic areas with more MHS transactions host more gun trafficking activity.

5.   Guns Sold by a Dealer With a High Trace Count

Another possible indicator that a gun has been trafficked is if it was sold by a licensed dealer to whom many other crime guns have been traced.[138] The underlying rationale is that many dealers who sell a disproportionately large number of guns that end up in criminal hands are corrupt dealers who knowingly or negligently sell guns to criminal consumers, unlicensed traffickers, or straw purchasers. The Attorney General of New York, Andrew Cuomo, made it clear during his 2006 election campaign that his planned policies for dealing with illegal guns were based on the belief that high trace counts indicate illegal behavior by gun dealers: "A wave of illegal guns has been breaking over New York for years. Incredibly, 1 percent of gun dealers account for the majority of illegal guns [that is, traced guns]. We need to crack down on their illegal behavior and put them out of business."[139]

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135.   Koper, *supra* note 10, at 758.
136.   Koper nevertheless asserted that MHS handguns were "at elevated risk for criminal use." *Id.* at 769. But this was true only within the tiny share (less than 1 percent) of all handguns that were recovered by police within one year of first retail sale, and the even smaller share of Maryland-sold guns that were recovered in nearby Washington, D.C. *Id.* at 761, 767.
137.   Garen J. Wintemute et al., *Risk Factors Among Handgun Retailers for Frequent and Disproportionate Sales of Guns Used in Violent and Firearm Related Crimes*, 11 INJ. PREVENTION 357, 361 (2005).
138.   *E.g.*, Pierce et al., *supra* note 10.
139.   Andrew Cuomo, The Role of the Attorney General, N.Y. L.J., Nov. 1, 2006, at 7.

The fact that many crime guns are traced back to a licensed dealer may appear damning, but for most such dealers, there are perfectly legitimate explanations for their high trace counts. First, if a dealer has a higher sales volume, it necessarily implies a larger number of guns at risk of coming into criminal possession through channels (such as theft from the owner) that are beyond the dealer's control. Thus, merely operating a successful business will increase the chances that a dealer will register a high trace count. A study of California FFLs found that just 11.7 percent of dealers accounted for 85.5 percent of traced crime handguns. This might suggest, as Mr. Cuomo apparently believed, that many of these FFLs must be criminal or irresponsible dealers—until one learns that these same dealers also accounted for 81.5 percent of all handgun sales.[140] That is, their share of crime guns was only slightly higher than one would expect if the FFLs were lawful and responsible dealers, and sheer sales volume accounted for their high trace counts. A dealer-level analysis likewise found that sales volume alone accounted for most of the variation in dealers' trace counts.[141]

Second, some FFLs do business in areas with higher crime rates, which leads to a larger share of the dealer's guns being stolen from their lawful purchasers, used in crimes, recovered by police, and traced by ATF. Thus, some or all of the variation in dealer trace counts that is not due to variation in sales volume may be attributable to variation in gun theft rates in the areas served by the FFLs. A recent dealer-level study imperfectly tested this idea. Wintemute and his colleagues analyzed predictors of dealer trace rates, but tested the effects only of types of crimes that rarely involve gun theft; the authors did not report any findings for the impact of rates of burglary, a crime that does often result in the theft of firearms. Among the crime types that they tested, the one that came closest to a property crime was robbery, and this was the one crime rate found to be significantly related to dealer trace rates—dealers in cities with higher robbery rates had higher trace rates.[142]

Consonant with these observations, ATF has long acknowledged that most licensed dealers to whom crime guns have been traced have been found to have been "operating within the confines of Federal law, and the vast majority of the illegal acts relating to these firearms occurred on the part of the individual purchasers" and not the dealers.[143] Even Philip Cook and

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140.   *See* Wintemute et al., *supra* note 137, at 360.
141.   Garen J. Wintemute, Research Letter, *Relationship Between Illegal Use of Handguns and Handgun Sales Volume*, 284 JAMA 566, 567 (2000).
142.   *See* Wintemute et al., *supra* note 137, at 360 tbl.4.
143.   CONCENTRATED URBAN ENFORCEMENT, *supra* note 20, at 62.

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Anthony Braga,[144] who strongly favor using tracing to uncover trafficking, conceded that "the number of traces to a particular FFL is only a rough indicator of the likelihood that the FFL is engaging in negligent or criminal sales practices."[145] Even this weak endorsement of trace counts as an indicator of trafficking, however, cannot be justified, since the ability of high trace counts to efficiently identify corrupt FFLs has never been empirically demonstrated.

6.   Obliterated Serial Number (OSN)

ATF is typically circumspect in its claims about the validity of the trafficking indicators it employs, for example, stating that short TTR "*suggests* illegal diversion" or that "acquisition of handguns in multiple sales *can* be" a trafficking indicator. In sharp contrast, ATF flatly states that "the obliteration of the serial number on a crime gun *is* a key criminal indicator of trafficking,"[146] and that "crime guns with obliterated serial numbers are *likely* to have been trafficked."[147] Braga and Pierce echo this assessment, unequivocally describing OSN as "a clear indicator of gun trafficking."[148] An OSN probably is the strongest available indicator of trafficker involvement in a gun's movement, since there are powerful motives for traffickers to efface serial numbers, while few people who are not traffickers have equally strong reasons for doing so. Obliteration not only definitively establishes that a criminal possessed the gun at some time (effacing a serial number is itself a crime), but also constitutes strong evidence that some past possessor wanted to obstruct the tracing of the gun, and thereby prevent it from being linked with past, presumably illegal, transfers. Traffickers would clearly want to impede tracing that could link them with their criminal associates, such as straw purchasers or a corrupt licensed dealer who supplied their guns. High-volume traffickers would be especially strongly motivated to impede tracing, since the more guns that one sells, the higher the risk that some of them can be traced back to the trafficker after being used in a crime.

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144.   *See* Cook & Braga, *supra* note 18, at 277–309.
145.   *Id.* at 302.
146.   U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, CRIME GUN TRACE REPORTS (1999): NATIONAL REPORT IX (2000).
147.   U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, *supra* note 12, at 8 (emphasis added).
148.   Cook & Braga, *supra* note 18, at 737; *see also* Koper, *supra* note 10, at 753 (noting that obliterated serial numbers are "an obvious flag for potential trafficking").

D.   Biases in Samples of Traced Guns

Experts have repeatedly concluded that the guns traced by ATF are not a representative sample of crime guns, and cannot provide a reliable picture of the modes of acquisition most frequently used by criminals or the paths of distribution that crime guns most often follow.[149] For example, the National Research Council's Committee to Improve Research Information and Data on Firearms flatly concluded that "trace data cannot show whether a firearm has been illegally diverted from legitimate firearms commerce."[150] It further concluded that studies based on this data "cannot show what happened in between [the first retail sale and recovery by law enforcement]: whether a firearm was legitimately purchased and subsequently stolen, sold improperly by a licensed dealer, or any other of a myriad of possibilities."[151] Even ATF has never explicitly claimed that traced guns are representative of crime guns or that they show the typical ways that guns are diverted to criminals. Unfortunately, many scholars have not taken these caveats sufficiently seriously, and have repeatedly drawn conclusions about the trafficking of crime guns, when their supporting data pertained only to nonrandomly selected subsets of guns that were traced.[152]

The problem is not merely that traced guns do not constitute a random sample of crime guns, and thus might be unrepresentative of crime guns generally. Rather, the processes by which guns are selected for tracing are known to systematically bias samples of crime guns in ways that tend to exaggerate the share of guns characterized by putative trafficking indicators. The biased selection occurs at two stages: (1) when police choose to request ATF traces for some guns and not others, and (2) when ATF is able to successfully trace some guns submitted for tracing but not others.[153] When police recover crime guns, their primary motive for submitting the guns for tracing is to help identify possible traffickers (and occasionally other types of criminals). It therefore is sensible for law enforcement officers to favor tracing guns that

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149.   U.S. CONG. RESEARCH SERV., "ASSAULT WEAPONS": MILITARY-STYLE SEMIAUTOMATIC FIREARMS FACTS AND ISSUES, H.R. REP. NO. 92-434 at 65 (1992); COMM. ON LAW & JUSTICE, NAT'L RESEARCH COUNCIL, FIREARMS AND VIOLENCE: A CRITICAL REVIEW 40 (Charles F. Wellford et al. eds., 2004) [hereinafter FIREARMS AND VIOLENCE]; Kleck, *supra* note 27, at 29–32.

150.   FIREARMS AND VIOLENCE, *supra* note 149, at 40.

151.   *Id.*

152.   *See, e.g.,* Christopher S. Koper, *Federal Legislation and Gun Markets: How Much Have Recent Reforms of the Federal Firearms Licensing System Reduced Criminal Gun Suppliers?,* 1 CRIMINOLOGY & PUB. POL'Y 151, 155, 175 (2002); Pierce et al., *supra* note 10; Travis & Smarrito, *supra* note 116, at 800.

153.   U.S. CONG. RESEARCH SERV., *supra* note 149; FIREARMS AND VIOLENCE, *supra* note 149.

show initial indications of trafficker involvement. For example, if the gun's serial number was obliterated, trafficker involvement is more likely. Likewise, if the criminal who possessed the gun when it was seized had an out-of-state driver's license, it is more likely that the gun also originated out of state. This in turn could suggest that the gun was moved across state lines by a gun smuggler. There might also be a preference for tracing newer models of guns, or guns that, based on limited wear, look newer, since tracing older guns has less investigative value—it is unlikely that identifying the person who bought a gun when it was new ten or twenty years ago would help identify a current trafficker. ATF has explicitly acknowledged that there is more law enforcement value in tracing newer guns: "[S]hort time-to-crime guns have the most immediate investigative potential for law enforcement officials because they are likely to have changed hands less frequently."[154]

One implication of this bias in favor of guns with a short TTR is that unwary analysts may misinterpret data on samples of traced guns as indicating that a large percentage of crime guns move directly from retail sale as new guns into the hands of criminals, even if the large share of guns with a short TTR is largely a reflection of the fact that police see little value in tracing older guns. Even sophisticated consumers of trace data have fallen into this trap. Although in other ways skeptical about the value of trace data, the members of National Research Council's Committee to Improve Research Information and Data on Firearms were convinced that one could somehow infer from trace data that crime guns that moved from other states into cities with tight gun regulations "are imported directly after the out-of-state retail sale"[155] (uncritically citing the conclusions of Cook and Braga[156]). In fact, trace data can neither establish that such guns were deliberately imported for purposes of illegal sale (rather than merely moved along with their owner's other possessions), nor that a large share of them were moved immediately after retail sale.

Samples of guns submitted for tracing may also under-represent guns with in-state origins because law enforcement personnel in states with their own gun-registration systems can use those systems to trace in-state guns, turning to ATF mostly for tracing of out-of-state guns along with a few in-state guns that were not successfully traced by the state's databases. Such a systematic bias would artificially inflate the out-of-state share.[157] Police may

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154.   U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, *supra* note 40, at xii.
155.   FIREARMS AND VIOLENCE, *supra* note 149, at 80.
156.   Cook & Braga, *supra* note 18.
157.   *See* Kleck, *supra* note 27, at 32; JEFFREY A. ROTH & CHRISTOPHER S. KOPER, IMPACT EVALUATION OF THE PUBLIC SAFETY AND RECREATIONAL FIREARMS USE PROTECTION ACT OF

also prefer to trace guns that they suspect came from another state simply because they believe, correctly or not, that a large share of crime guns in their city were smuggled from out-of-state, and they want to identify the sources.

Further, types of guns that are of especially strong political interest and subject to heightened media attention may also be overrepresented among guns selected by police for tracing. Failure to fully appreciate this bias in traced-gun samples has lead to unwarranted conclusions in past research. For example, Travis and Smarrito claimed that assault weapons (AWs) were "disproportionately involved in criminal activity," based entirely on samples of traced guns,[158] which over-represent AWs.[159] Likewise, Christopher Koper and Jeffrey Roth concluded that national trends in trace requests suggest that criminal use of AWs declined after the federal assault weapons ban was passed.[160] In sharp contrast, Koper's and Roth's data on all AWs recovered by police (not just those submitted to ATF for tracing) indicated that there were no significant declines in the AW share of crime guns in the wake of the federal ban.[161] Thus the decline in AW trace requests may merely have been an artifact of a decline in police interest in tracing AWs once the AW problem was "solved" by passage of the federal AW ban and once news media interest in the issue declined. Although this hypothesis was dismissed by Koper and Roth, it is perfectly consistent with the authors' own observation that the decline was weaker in states that already had their own AW laws,[162] where passage of the largely redundant federal ban would presumably have been of less significance or popular interest.

In addition to police preferences for submitting trace requests on guns with certain traits, ATF has its own policies concerning which guns it will trace, and these policies further bias samples of traced guns. At various times in the past, ATF would not routinely trace guns more than five (or ten, or twenty) years old, which skewed the distribution so that nearly all traced guns were relatively new, no matter how common older guns were in the entire population of recovered crime guns. For example, in a 1999 report, ATF

1994, at 59 (1997); Eleanor Weber-Burdin et al., Weapons Policies: A Survey of Police Department Practices Concerning Weapons and Related Issues 4–9 (1981) (unpublished report to the U.S. Department of Justice, University of Massachusetts, Amherst); Zimring, *supra* note 10, at 105 n.2.

158.   Travis & Smarrito, *supra* note 116, at 800.

159.   *See* KLECK, *supra* note 119, at 112.

160.   The assault weapons ban was Title XI of the Violent Crime Control and Law Enforcement Act of 1994, Pub. L. No. 103-322, 108 Stat 1796 (codified as amended in scattered sections of 42 U.S.C.). *See* Christopher S. Koper & Jeffrey A. Roth, *The Impact of the 1994 Federal Assault Weapons Ban on Gun Markets*, 18 J. QUANTITATIVE CRIMINOLOGY 239, 256–59 (2002).

161.   *See id.* at 260–61.

162.   *See id.* at 257–59.

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stated that their National Tracing Center's "policy was not to trace firearms manufactured before 1990, unless specifically requested by a law enforcement management official"[163]—that is, no tracing of guns more than nine years old. Despite widespread, decades-old awareness of this censoring of older guns from trace samples, scholars have continued to insist, based solely on firearms tracing data, that few crime guns are older guns,[164] or that crime guns are "imported [into tight control cities] directly after the out-of-state retail sale."[165]

In sum, the process of selecting guns for tracing results in data that over-represent guns that are relatively new (and therefore have a shorter TTR), have out-of-state origins, or have other traits that are associated with these characteristics. That is, samples of guns successfully traced or submitted for tracing overrepresent guns that look like they were trafficked. This problem is routinely ignored by those who use trace data to support a claim that trafficking is important in supplying guns to criminals. For example, Glenn Pierce and his colleagues conclude that crime guns are disproportionately new compared to the total stock of guns, as judged by manufacture and importation data.[166] Their data, however, pertained only to samples of traced guns, which systematically excluded nearly all of the older crime guns.

It has been hinted (though never explicitly stated) that the unrepresentative nature of traced gun samples was, beginning around 1997, largely eliminated in cities participating in the ATF Youth Crime Gun Interdiction Initiative (YCGII) program, because these cities promised to trace "comprehensively" (i.e. request traces on all the guns that their police recovered). Some scholars appear to have taken it on faith that all police departments that promised to perform comprehensive tracing actually did so.[167] However, these scholars typically do not consider whether YCGII cities do actually submit trace requests on all, or nearly all, recovered crime guns. Rather, they draw conclusions about

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163.   U.S. Bureau of Alcohol, Tobacco & Firearms, *supra* note 12, at 19.

164.   *See, e.g.*, Braga et al., *supra* note 10, at 331–33 (favorably listing studies that use firearms trace data to conclude that "recovered crime guns tend to be quite new"); Cook et al., *supra* note 10, at 62–63 ("[W]e conclude that *most* guns used in crime . . . have been acquired relatively recently.") (citing Zimring, *supra* note 10); Zimring, *supra* note 10, at 95–96 (supporting the "new guns" hypothesis with a study of "federally initiated traces."). Both Braga et al. and Cook et al. note some limitations of trace data, but then proceed to draw precisely the same conclusions that would follow in the absence of their caveats.

165.   Firearms and Violence, *supra* note 149, at 80.

166.   Pierce et al., *supra* note 10, at 394.

167.   *See, e.g.*, Cook & Braga, *supra* note 18, at 286; Koper, *supra* note 10, at 759 (stating that because Baltimore and Washington, D.C. have a comprehensive tracing policy, the cities can provide "complete data on guns recovered in those jurisdictions"); Pierce et al., *supra* note 10, at 397; Wintemute et al., *supra* note 137, at 361 ("Most traced guns in California come from cities with mandatory tracing policies, so within-jurisdiction selection bias should be minimal.").

crime guns in general based solely on analyses of traced guns—conclusions that logically follow from the evidence only if one assumes that YCGII cities actually do trace comprehensively, thereby guaranteeing that traced gun samples accurately represent the population of all recovered crime guns.[168]

This assumption, however, is clearly false for many of the YCGII cities, and remains unsubstantiated for the rest. ATF has repeatedly acknowledged that "the effort to achieve comprehensive tracing has not been fully institutionalized,"[169] that it "cannot determine definitively whether all recovered guns are being traced,"[170] that "the tracing of guns with obliterated serial numbers is not conducted consistently by law enforcement agencies,"[171] and that "the extent of program implementation varies from one jurisdiction to another"[172]—something that obviously could not be true if implementation was 100 percent in all participating cities.

In 1999 ATF conducted a survey of YCGII police departments in order to determine the completeness of tracing, and "about half" of the thirty-eight cities participating at the time in the YCGII program did not even respond to the survey. ATF explicitly acknowledged that ten of the remaining nineteen (or so) cities were tracing less than 100 percent of recovered guns.[173] ATF has not repeated this evaluation effort since 1999. Even the figures on tracing rates provided to ATF by these reporting agencies were not substantiated by ATF. ATF did not perform any independent assessments of tracing levels for any of the YCGII agencies, for example by performing their own audits of police department gun files in order to establish the share of recovered guns that matched up with trace requests submitted to ATF. Thus, the actual completeness of tracing remains unknown for most YCGII cities. In addition, there is still no firm evidentiary basis for the claim that YCGII eliminated or even substantially reduced the sample bias due to the preferences of police officers for requesting traces on guns displaying various presumed signs of trafficking.

Even if police really did submit all recovered guns for tracing, only an unrepresentative subsample could be successfully traced to the point where the presence or absence of various potential indicators of trafficking can be established. For example, a gun must be successfully traced to its first retail

168. *See, e.g.*, Braga et al., *supra* note 10, at 331; Cook & Braga, *supra* note 18, at 303–07; Koper, *supra* note 10, at 759; Pierce et al., *supra* note 10, at 397.

169. U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, *supra* note 12, at A1.

170. U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, *supra* note 40, at 67.

171. *Id.* at 50.

172. U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, *supra* note 12, at B3.

173. U.S. BUREAU OF ALCOHOL, TOBACCO AND FIREARMS, DEP'T OF THE TREASURY, CRIME GUN TRACE REPORTS (1999): NATIONAL REPORT B4, B5 (2000).

sale in order to establish whether this sale occurred in a state different from the one in which it was recovered, or to determine how long ago the sale occurred, thereby establishing TTR. ATF, however, will not even initiate traces on older guns unless a law enforcement executive makes a special request, or the dealer that sold the gun has gone out of business and the records of their transfers can be found in ATF's out-of-business dealer files.[174] Thus, among the 88,570 guns for which police in forty-four YCGII cities requested a trace in 2000, ATF did not even begin a trace for 12.8 percent of them, in most cases because the gun was too old. Among the guns for which ATF did initiate a trace, another 33.6 percent could not be successfully traced to their first retail purchaser. And for at least 10.7 percent of all trace requests, a trace could not be completed to the first retail purchaser for reasons clearly related to the gun being older (it had been produced or imported by a manufacturer or importer no longer in business, the twenty-year record retention period had expired, or records were otherwise no longer available).[175]

Thus, even after the advent of YCGII, it was still impossible to successfully trace about half of the guns submitted for tracing. In addition, unknown numbers of other guns recovered by police were never submitted for tracing. As such, there remained ample reasons to suspect systematic bias in the data obtained from samples of successfully traced guns. In particular, the percent of recovered guns that appeared to be fairly new (have a short TTR), is overstated as a result of the systematic exclusion of older guns from those submitted for tracing, and from those for which a trace successfully was completed. On the other hand, because this problem is inherent in the national ATF tracing system, the inability to trace older guns operates to a similar degree in all localities. Thus, although traced gun samples overstate the absolute prevalence of supposed trafficking indicators among crime guns, use of such samples does not necessarily distort comparisons across different areas. Trace data may still provide a basis for macro-level indicators of the relative prevalence of trafficking between cities.

III.   A TENTATIVE ESTIMATE OF THE TRAFFICKING SHARE OF CRIME GUNS

As previously noted, the guns known to have been trafficked as a result of law enforcement investigations comprise only a tiny share (probably under

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174.   U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, *supra* note 40, at 68.
175.   *See id.* at 25–27, 68.

1 percent) of the guns acquired by criminals.[176] This clearly establishes that ATF enforcement efforts impact only a tiny share of the flow of guns to criminals. However, it cannot establish the trafficker-supplied share of crime guns since some traffickers are not caught, and the authorities may underestimate the number of guns trafficked by those who are apprehended. One can instead approach this issue by considering the prevalence of stronger trafficking indicators among traced guns. Suppose, for the sake of argument, that all trafficked guns had OSNs, and all guns with OSNs had been trafficked. National tracing data indicate that less than 1.6 percent of traced guns have OSNs, suggesting that few crime guns were trafficked. When ATF examined a sample of recovered handguns from all 46 YCGII cities that was limited to just those with an extremely short time-to-recovery (TTR) of one year or less—which, according to ATF doctrine are especially likely to have been trafficked—only 1.6 percent of these handguns had an OSN.[177] If one takes into account the fact that some guns with OSNs were not trafficked, then the estimated trafficked share would be still lower than 1.6 percent— probably under one percent.

Moreover, if one only labeled as "trafficked" guns that possess other indicators in addition to an OSN and an extremely short TTR, the trafficking share would be lower still. For example, ATF found that only 0.4 percent of crime handguns with a TTR under one year that were traced in 2000 had an OSN and were purchased as part of a multiple handgun sale (MHS).[178] Because this sample was limited to those with TTRs under one year, it was biased in favor of guns with supposed trafficking indicators. Further, since crime guns with a TTR under one year comprised only 15 percent of all traced guns,[179] and just 0.4 percent of these fast-TTR handguns had an OSN and were part of a MHS, only about 0.06 percent, or one in 1,667, traced guns had all three of these putative indicators of having been trafficked.

In any case, trace data are fully consistent with the hypothesis that traffickers supply less than one percent of crime guns. Certainly, there is no affirmative evidence that traffickers supply even this large a share of crime guns. Nevertheless, since it is possible that substantial numbers of trafficked guns never had their serial numbers obliterated, the trafficked share could be larger than OSN prevalence suggests. Further, even small numbers of trafficked guns might influence the share of criminals with guns, if the trafficking was

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176.   *See supra* Part I.C, at 1245–46.
177.   U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, *supra* note 40, at 50, 52.
178.   *Id.* at 50, 52 tbl.21.
179.   *Id.* at 30.

concentrated in areas where significant numbers of criminals had no satis-factory alternative sources of guns. Thus, it remains an open question whether trafficking levels affect crime rates—a question that can be tested with an analysis of empirical data. This analysis, however, requires valid measures of trafficking.

IV.   NEW CITY-LEVEL EVIDENCE ON GUN TRAFFICKING

A.   Methods of the Present Study

We wanted to first evaluate the utility of ATF trace data for measuring the prevalence of gun trafficking activity in cities, so we tested various indicators of whether (1) individual crime guns had been trafficked, or (2) individual FFLs were involved in trafficking, in order to determine which, if any, could be used as city-level indicators of the prevalence of gun trafficking. Then, assuming that some of the indicators were valid, we sought to explore (1) the conditions that favor higher trafficking levels, (2) the impact of gun trafficking on gun possession among criminals, and (3) the impact of gun trafficking on violent crime rates.

Either of two likely possibilities regarding the validity of gun trace-based indicators of gun trafficking may be true. First, all of these indicators might be invalid, including even the one in which the most faith is placed, the prevalence of OSNs. If this is so, this means that the case for the concentrated gun trafficking model, which relies almost entirely on trace data, is fundamentally unsound and therefore cannot be taken seriously. Alternatively, some trace-based indicators—in particular, the prevalence of OSNs among recovered crime guns—might be relatively valid and useful as measures of the prevalence of gun trafficking. If this is the case, the concentrated gun traf-ficking model still fails, because our analysis of patterns among putative trafficking indictors shows (1) that most of them have little correlation with each other (suggesting that, even if some are valid indicators of gun trafficking, they are mostly measuring different things), and (2) that even the best indicators show no significant positive association with measures of gun availability among criminals or crime rates (suggesting that even if some sort of gun trafficking is being validly measured, it has no measurable effect on criminal gun possession or crime rates).

ATF has released detailed reports on fifty YCGII cities, describing the guns submitted by their police departments for tracing in 2000.[180] Our tentative working assumption was that the larger the share of these guns that displayed putative trafficking indicators, the larger the share of local crime guns that was supplied by traffickers. That is, we initially assumed that biases in samples of traced guns are sufficiently similar across YCGII cities to permit meaningful comparisons of the relative prevalence of putative trafficking indicators across those cities. We began by examining bivariate correlations among the indicators. If the measures all reflect levels of trafficking, they should have strong bivariate correlations with each other. Then we conducted a principle components analysis to see if the indicator variables all reflect, to varying degrees, a single underlying factor. Finally, we estimated regression models to estimate the impact of apparent trafficking levels (based on putative trace-based indicators) on criminal gun possession and on violent crime rates.

B.   Findings

Table 2 lists the variables in the analysis, including the potential city-level indicators of the prevalence of gun trafficking, while Table 3 displays the weighted correlations among the trafficking indicators. Each YCGII city is weighted by the number of trace requests it submitted to ATF, since this quantity purportedly equals the total number of crime guns recovered by the police in that city. Table 3 also includes the percent of suicides committed with guns (PSG), which has been shown to be a highly valid proxy for measuring differences in gun ownership levels across areas.[181] PSG is used to test the hypothesis that there will be less trafficking in cities where local, predominantly lawful gun ownership is already high, and criminal demand can therefore be met by guns stolen from local residents. If this hypothesis is correct, PSG should be negatively related to any variables that are valid indicators of trafficking prevalence. Table 3 also includes a gun theft rate variable derived from the Stolen Gun Files of the FBI's National Criminal Information Center.[182] These data were available only at the state level, so they pertain to the state in which each city is located. The gun theft counts are for a two-year period from 1999 to 2000, so they were divided in

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180.   These data are available on the Web at BUREAU OF ALCOHOL , TOBACCO, & FIREARMS, *supra* note 40.

181.   COOK & LUDWIG, *supra* note 34; Gary Kleck, *Measures of Gun Ownership Levels for Macro-Level Crime and Violence Research*, 41 J. RES. CRIME & DELINQUENCY 3, 8–19 (2004).

182.   *See* AMS. FOR GUN SAFETY FOUND., STOLEN FIREARMS: ARMING THE ENEMY 16, 17 tbl.3 (2002) (report based on NAT'L CRIME INFO. CTR., FEDERAL BUREAU OF INVESTIGATION, U.S. DEP'T OF JUSTICE, STOLEN GUN FILE RECORDS (1999–2000)).

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half to produce an annual average, and then divided by the state's population (in 100,000s). No gun theft data was available for the District of Columbia (D.C.), but since D.C. has lower-than-average gun ownership but higher-than-average crime rates, it was assigned the national average gun theft rate as a reasonable approximation.

TABLE 2. VARIABLES IN THE CITY ANALYSIS
(Consolidated data from 50 cities, weighted by number of trace requests)

Variable	Description	Mean	Standard Deviation
OSN	Percent recovered guns with obliterated serial number	4.86	4.51
OUTSTATE	Percent recovered guns first sold in another state	32.97	19.94
DLR250ML	Percent recovered guns first sold by FFL ≥ 250 miles away	24.02	17.74
POSNOTBY	Percent recovered guns possessed by person not 1st buyer	88.84	5.93
TTRU1YR	Percent recovered guns with time-to-recovery under 1 year	14.46	5.26
TTRU3YR	Percent recovered guns with time-to-recovery under 3 years	30.96	8.86
TTRMEDN	Median time-to-recovery among recovered guns	6.00	1.42
DELR5PTR	Percent recovered guns traced to FFL with 5+ traces	52.45	16.27
DLR10PTR	Percent recovered guns traced to FFL with 10+ traces	42.67	18.91
DLR25PTR	Percent recovered guns traced to FFL with 25+ traces	29.53	18.70
DISTANCE	Distance in miles, city center to nearest state border	74.39	89.19
BURGRATE	Burglaries known to police per 100,000 people	1269.15	498.51
PSG9498	Percent of suicides committed with guns, 1994-1998	51.51	13.16
TRAFVOLU	Number of traced guns with OSN per 100,000 people	15.01	14.82

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MURDRATE	Murders, nonnegligent manslaughters per 100,000 people	19.57	11.78
ASLTRATE	Aggravated assaults per 100,000 people	756.21	324.57
ROBRATE	Robberies per 100,000 people	534.83	222.04
PGH9902	Percent of homicides committed with guns	70.25	7.98
COPRATE	Sworn officers per 100,000 people	6027.13	9860.19
POVERTY	Percent population below poverty line	19.95	4.74
MFI	Median family income (dollars)	40950.40	7580.97
UNEMPLOY	Percent labor force unemployed	5.11	1.81
EDUC	Percent population age 25+ with high school diploma or higher	73.96	6.67
BLACK	Percent population African-American	34.99	21.46
HISP	Percent population Hispanic	19.31	17.66
AGE1824	Percent population age 18–24	11.20	1.52
OWNEROCC	Percent housing units occupied by owners	48.66	9.16
FEMHEAD	Percent of households headed by females	18.63	4.97
POPCHANG	Percent change in population from 1990 to 2000	5.85	13.60
POPCITY	Resident population of city (in 100,000s)	15.08	19.18
DENSITY	Persons per square mile	7112.03	6319.20
SOUTH	City located in former slave-owning state	0.43	0.50
STORES	Retail establishments per 100,000 people	375.62	100.64
ONEGUN	State law limiting handgun purchases to one per month (0=no, 1=yes)	0.06	0.23
REGISTER	State law requiring registration of handgun purchases (0 = no, 1 = yes)	0.28	0.12

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| PERMIT | State law requiring permit to purchase handgun (0 = no, 1 = yes) | 0.31 | 0.47 |
| WAITPER | Days buyer must wait before taking delivery of handgun | 1.71 | 3.02 |

The correlations in Table 3 indicate that many of the potential traf-ficking indicators are not significantly correlated with each other, and some are even negatively correlated. For example, if one tentatively assumes that the percent of crime guns that have an OSN is a strong indicator of trafficking, as both ATF and scholars agree, one finds that cities where many crime guns can be traced back to retail dealers with high trace counts actually have less trafficking, as measured by the percent of recovered guns with OSNs. This is not what one would expect if one assumed that many high trace count dealers were involved in trafficking. On the other hand, these findings are fully compatible with the hypothesis that high trace counts primarily reflect high sales volume, since there is a strong positive correlation between the share of crime guns sold by dealers with high trace counts and the city's gun ownership rate, and thus its volume of gun sales to the noncriminal public. That is, these correlations suggest that indicators based on high dealer trace counts are more likely to reflect higher volumes of lawful gun sales than the involvement of corrupt licensees in trafficking.

Consistent with this idea, one of the strongest (and highly significant) correlations in the table is between PSG and OSN. This supports the hypothesis that the higher a city's local gun ownership level, the less its gun trafficking activity. Where more guns are owned, more guns will be stolen, other things being equal, which results in more guns circulating among criminals. A large volume of stolen guns competes with guns sold by traffickers and depresses black market prices, reducing both the profit incentive for traffickers and the need for their services. This interpretation is directly supported by the significant ($r = -0.517$) correlation between the gun theft rate and OSN prevalence among traced crime guns; where more guns are stolen, there is less trafficking. These correlations can also be viewed as indications of the construct validity of the OSN indicator as a measure of traf-ficking activity: it correlates strongly with variables (gun ownership levels and gun theft rates) with which it should be correlated if our hypothesis is correct.[183]

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183.   *See* JUM C. NUNNALLY, PSYCHOMETRIC THEORY 86–87 (1967).

TABLE 3. CORRELATIONS AMONG POTENTIAL GUN TRAFFICKING INDICATORS—
PERCENT OF RECOVERED GUNS WITH INDICATED TRAIT
(Consolidated data from 50 cities, weighted by number of trace requests)

	1	2	3	4	5	6	7	8	9	10	11	12	13
1 Obliterated Serial Number	1	.689 / .000	.660 / .000	.442 / .001	-.183 / .102	-.151 / .148	.158 / .136	-.310 / .014	-.290 / .021	-.200 / .082	-.425 / .001	-.517 / .006	-.695 / .000
2 Out of State Origin		1	.918 / .000	.656 / .000	-.376 / .004	-.342 / .008	.354 / .006	-.674 / .000	-.635 / .000	-.544 / .000	-.492 / .000	-.442 / .001	-.684 / .000
3 Dealer 250+ Miles Away			1	.575 / .000	-.426 / .001	-.419 / .001	.456 / .000	-.651 / .000	-.615 / .000	-.531 / .000	-.258 / .035	-.560 / .000	-.729 / .000
4 Possessor Not 1st Purchaser				1	-.370 / .004	-.350 / .006	.300 / .017	-.510 / .000	-.512 / .000	-.472 / .000	-.484 / .000	-.254 / .037	-.336 / .008
5 Time-to-Recovery Under 1 Years					1	.968 / .000	-.936 / .000	.505 / .000	.477 / .000	.436 / .001	-.102 / .241	.481 / .000	.381 / .003
6 Time-to-Recovery Under 3 Year						1	-.975 / .000	.496 / .000	.484 / .000	.447 / .001	-.177 / .109	.526 / .000	.374 / .004
7 Median Time-to-Recovery							1	-.522 / .000	-.513 / .000	-.478 / .000	.188 / .096	-.588 / .000	-.398 / .002
8 Dealer Has 5+ Traces								1	.979 / .000	.927 / .000	.193 / .089	.370 / .004	.404 / .002
9 Dealer Has 10+ Traces									1	.956 / .000	.168 / .122	.358 / .005	.357 / .005
10 Dealer Has 25+ Traces										1	.096 / .255	.256 / .036	.226 / .057
11 Distance from City to State Border											1	.035 / .406	.189 / .094
12 State Gun Theft Rate												1	.660 / .000
13 % Suicides With Gun (PSG)													1

The OSN measure is moderately correlated with measures of the share of crime guns that traveled into the jurisdiction from distant locales—the percent first sold out of state, and the percent sold by FFLs over 250 miles from the city where the crime gun was recovered. These two "distant-origin" variables are almost perfectly correlated with each other, and are basically two ways of measuring the same underlying trait. The distant-origin measures, however, are ambiguous because they also reflect the geographical location of

the city. We measured the distance from each city's center to the nearest state border, and found significant negative correlations between this distance and the percent of crime guns first sold out of state or by a distant FFL. In other words, a city may have a larger share of its crime guns coming from another state simply because it is located closer to that state. Other things being equal, the closer a city is to a given state, the more of its migrants originate from that state. Migrants bring their possessions, including their guns, with them, and some of the migrants are burglarized in their new homes. Conse-quently, a city with many residents who moved there from state X is likely to have more guns that had been lawfully purchased in state X show up among the guns recovered from criminals in that city. Consistent with this, ATF trace data indicate that, among crime guns originating out of state, the state that guns are most likely to have come from is, other things being equal, the nearest state among those with larger populations.[184] Thus, the distant-origin indicators may reflect both a city's proximity to other states and trafficking prevalence. Nevertheless, distant origins of crime guns may be the next-best trafficking indicator, after OSN prevalence.

Among the remaining potential trafficking indicators, only one measure showed even a modest correlation with the OSN measure. The percent of guns whose criminal possessor was not the original retail buyer had a significant ($r=0.44$) correlation with OSN. It was also significantly correlated with the distant-origin measures. This is consistent with the expectation that the further a gun traveled to a city, the more likely it is that the gun passed through the hands of multiple possessors.

The measures of the prevalence of fast-TTR (TTR less than one year) guns had *no* significant correlation with OSN. Excluding their correlations with each other, they also were not strongly related to any other indicators. Indeed, many of their correlations were even negative. Thus, even if one rejected the validity of the OSN indicator, one would still have to conclude that there is little support for TTR as a trafficking indicator. The only indicators with which the TTR variables were moderately ($0.4<r<0.6$) and significantly correlated were those reflecting the share of crime guns linked to dealers with high trace counts. Both of these types of indicators appear to be poor measures of trafficking prevalence. Instead, fast-TTR and high-FFL trace counts are more likely to be indicators of higher gun theft rates, since the correlation between the state gun theft rate and median TTR was significant ($r=-.588$). It is all the more remarkable that this correlation is as strong as it is given the considerable error in the measurement of gun theft;

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184.   *See* U.S. BUREAU OF ALCOHOL, TOBACCO, & FIREARMS, *supra* note 40.

most thefts are not reported to the police,[185] and this rate pertained to theft in the surrounding state rather than just the city itself. In any case, the rapid movement of guns into criminal hands is far more strongly correlated with gun theft rates than with putative gun trafficking indicators.

TABLE 4. PRINCIPLE COMPONENTS ANALYSIS OF POTENTIAL
TRAFFICKING INDICATORS
(Factor loadings of rotated solutions)[186]

| Analysis | Exploratory Analysis | | | Confirmatory Analysis |
| | (No Constraints on number of factors) Component | | | (Constrained to one factor) |
	1	2	3	1
OSN	.011	−.030	.898	−.505
OUTSTATE	−.405	−.150	.851	−.807
DLR250ML	−.365	−.249	.815	−.816
POSNOTBY	−.353	−.184	.630	−.671
TTRU1YR	.208	.944	−.171	.743
TTRU3YRS	.214	.962	−.137	.738
TTRMDN	−.249	−.942	.136	−.748
DELR5PTR	.886	.272	−.324	.885
DLR10PTR	.912	.254	−.287	.870
DLR25PTR	.933	.226	−.183	.811

Next, we performed an exploratory factor analysis of all the potential indicators. We initially did not restrict the number of factors that could be extracted because we wanted to know whether all the items were indicators of a single underlying construct, presumably the prevalence of gun trafficking, and thus loaded on a single factor. The left side of Table 4 displays the results of a principle components factor analysis with varimax rotation. This analysis extracted three factors with eigenvalues greater than one, indicating that a single underlying factor was not sufficient to adequately explain the observed correlations among potential indicators. The first factor primarily reflects the prevalence of crime guns with fast TTRs, the second primarily reflects the prevalence of guns originating with dealers with high trace counts, and the third mainly reflects the prevalence of guns with OSNs

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185.   BUREAU OF JUSTICE STATISTICS, *supra* note 33 at tbl.93a.
186.   Principal component analysis, using varimax rotation with Kaiser normalization.

and guns that originated in distant locales. Whatever these indicators are measuring, they do not appear to be measuring the same thing. Prior research suggests that the third factor is the relatively more valid measure of trafficking of the three because it reflects the prevalence of a reputedly strong indicator, OSN prevalence, and other indicators correlated with OSN.[187] The first factor may simply be measuring higher sales volumes in some cities, which would lead to higher average trace counts among FFLs even in the absence of trafficking activity. The second factor may be an indirect measure of high gun theft rates, since the more often gun thefts occur, the faster guns move into criminal hands. Results were substantially the same when oblimax rotation, which does not assume that factors are orthogonal, was used: three factors were extracted, with the same clusterings of items.

The right side of Table 4 displays the results of a factor analysis in which the solution was constrained to a single factor, based on the a priori assumption that all the items were valid indicators of a single unmeasured trait, such as trafficking prevalence. These results also suggest that the items are measuring different concepts, since about half of the supposed trafficking indicators load positively on the factor and about half load negatively. Whatever the single underlying concept might be, the individual items do not measure this concept in the same direction. Cities with more of this underlying concept have, on the one hand, more guns with fast TTR and more guns from dealers with high trace counts, but, on the other hand, have fewer guns with an OSN, a possessor different from the original buyer, or distant origins. These results are hard to reconcile with the idea that all of these variables are indicators of gun trafficking. A few of them might be indicators, but most of them probably are not.

Another approach to assessing measurement validity is to select a criterion measure thought, on a priori grounds, to be the best measure available, and then measure correlations between this criterion and other potential measures.[188] If one tentatively accepted the a priori reasoning that pointed to OSN prevalence as the best available measure of the prevalence of trafficking, as well as the rather definitive endorsement by ATF and scholars of the validity of this trait as an indication that a gun had been trafficked, it could be treated as a criterion measure. Table 3 correlations indicated that, by this standard, the only other indicators with even moderate validity are the distant-origins measures—the percent of crime guns originating out-of-state and the percent originating with dealers from over 250 miles away. But even these

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187.   *See* Part II.C.6 at 1269.
188.   NUNNALLY, *supra* note 183, at 77–78.

variables share less than half their variation in common with OSN (r^2<.5), suggesting that they mostly measure something other than what OSN measures, and therefore should not be regarded as strong indicators of trafficking levels.

Because the validity of even OSN as a trafficking measure is debatable, the Table 5 multivariate analyses making use of this measure must be regarded as strictly exploratory. ATF states that police in YCGII cities do not consistently request traces on crime guns with OSNs,[189] though the same could probably be said of crime guns in general in these cities. These analyses are performed for the purpose of exploring the causes and consequences of higher trafficking levels if one accepts the validity of OSN as a measure of the prevalence of gun trafficking in a city.

Thus, we tentatively assumed that OSN prevalence among traced guns in a city measures the prevalence of gun trafficking, and we estimated weighted least squares models to investigate some of the possible determinants of gun trafficking levels, and the impact of gun trafficking on criminal gun ownership and crime rates. As in the previous analyses, cities were weighted by the number of crime guns for which traces were requested. Of course, if even this reputedly strong indicator of trafficking is not valid, it is highly unlikely that any of the other putative indicators are similarly valid. Therefore, the case for the importance of organized or high-volume gun trafficking collapses, since it is almost entirely based on analyses that assume the validity of these indicators.

TABLE 5. THE DETERMINANTS OF GUN TRAFFICKING LEVELS AND THEIR EFFECTS ON CRIMINAL GUN POSSESSION LEVELS AND CRIME RATES[190]

	Coefficients (Ratio of coefficient/standard error)				
	(1)	(2)	(3)	(4)	(5)
Dependent Variable:	OSN	PCTGHOM	Murder Rate	Robbery Rate	Assault Rate
Independent Variables:					
OSN (Gun Trafficking)		.366 (1.28)	–.006 (–0.53)	.022 (1.88)	.008 (0.54)

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189.   *See* U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, *supra* note 40, at 50.
190.   Cities were weighted by number of trace requests. Variables present in some crime rate models but not others were omitted because they were found to be unrelated to that specific crime rate.

PSG9498	−0.207 (−4.23)	0.253 (2.05)			
PCTGHOM			0.034 (6.07)	0.009 (1.45)	0.001 (0.15)
Murder Rate		0.501 (5.14)			
Burglary Rate		−0.005 (−2.28)			
ONEGUN	4.171 (1.94)	14.435 (−3.11)	0.843 (3.97)	0.588 (2.54)	0.309 (1.07)
REGISTER	0.653 (0.48)	−4.029 (−1.57)	−0.224 (−2.00)	−0.346 (−2.83)	0.309 (1.07)
WAITPER	−0.221 (−1.21)	0.311 (0.93)	0.015 (0.94)	0.015 (0.86)	0.044 (2.04)
PERMIT	1.338 (1.02)	0.394 (0.15)	0.180 (1.72)	0.254 (2.22)	0.236 (1.66)
POVERTY			0.029 (2.78)	0.042 (3.78)	
BLACK			0.012 (4.61)	0.005 (1.65)	0.011 (2.83)
HISPANIC					0.008 (1.88)
Constant	14.999	53.735	−0.626	4.404	5.778
R^2_A	0.493	0.477	0.828	0.646	0.300
Alternate trafficking proxy results: DLR250ML[191]		−0.050 (−0.58)	−0.004 (−1.16)	−0.004 (−0.95)	−0.007 (−1.39)

The resulting estimates are shown in Table 5. Column 1 displays estimates of a model of the percent of a city's crime guns recovered by police that had an OSN, treated here as a proxy for the prevalence of gun trafficking in the city. That is, the estimates address the question: What conditions

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191.   These are estimates from models including the same variables in each model that are shown in this table, but using DLR250ML as the trafficking proxy instead of OSN.

favor higher gun trafficking levels? They indicate, first, that the higher the gun ownership rate that prevailed in a city in the late 1990s, the lower the share of the city's crime guns recovered in 2000 that were supplied by traffickers. Second, none of four types of state laws regulating the purchase of firearms influence trafficking prevalence: laws limiting handgun purchases to one a month, laws requiring the registration of handgun purchases, laws requiring a permit to purchase guns, nor laws specifying a minimum number of days that a buyer must wait before taking delivery of a gun. All showed no relationship with the share of crime guns that were trafficked. When the gun theft rate was included in the model instead of the gun ownership measure, its coefficient was also significant and negative (b=−0.054, p<.01), indicating that where gun theft was more common, trafficking was less prevalent. Because gun ownership and the gun theft rate were highly correlated (r=0.66), however, both could not be included in the same model and still retain significant coefficients.

Column 2 of Table 5 reports estimates of a model of the prevalence of gun possession among criminals, measured as the share of homicides committed with guns.[193] The results indicate that trafficking, as measured by OSN, has no significant effect on the share of criminals in possession of guns. We also created a measure of the volume of trafficking, computed as the number of trace requests (purportedly the number of crime guns recovered by police), multiplied by the percent with an OSN. When this was included in the model instead of OSN, the results (not shown) were even less supportive (1-tailed, p =.438) of the hypothesis that trafficking levels affect gun possession levels among criminals.

The murder rate appears to have a significant positive effect on criminal gun possession, suggesting that more dangerous environments motivate more criminals to acquire guns for protection. This association, however, could also reflect a positive effect of criminal gun levels on murder rates. Laws regulating gun sales generally show no effect on criminal gun possession, with one notable exception: Laws limiting citizens to one handgun purchase per month, which are explicitly intended to reduce gun trafficking, appear to have a significant negative effect on gun possession among criminals. It is unlikely, however, that this reflects an actual effect of one-gun-a-month laws via their effects on trafficking, since these laws showed no effect on levels of trafficking (see Column 1). This negative association may instead reflect a negative

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193.   For a recent example of this measure's use as a measure of access to guns among criminals, *see* Cook & Braga, *supra* note 18, at 306–07.

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effect of gun ownership on the enactment of gun control laws. Gun levels among noncriminals are highly correlated with gun levels among criminals, and larger numbers of gun-owning voters discourage legislators from supporting new gun laws.[194]

Columns 3 through 5 report estimates of the parameters of models of rates of murder, robbery, and aggravated assault. All crime rates were expressed in terms of their natural logs, to reduce the skewness of their distributions. Because the Column 2 results indicated that trafficking levels have no effect on criminal gun possession levels, there is no obvious reason why trafficking should affect crime rates. The PCTGHOM (percent of homicides committed with guns) variable, however, is only an imperfect indicator of gun possession among criminals, so it remains possible that trafficking has some undetected impact on criminal gun possession, and thus on crime rates. The crime rate results nevertheless indicate that trafficking has no effect on rates of either murder or assault, but may have a marginally significant (1-tailed, $p=.034$) positive effect on robbery. Given the evidence that trafficking does not affect criminal gun levels or homicide or assault rates, this borderline-significant association with robbery may be nothing more than a product of random chance and a large number of hypothesis tests. The weakness of the associations between trafficking and either criminal gun possession or crime rates could, however, also be partly attributable to random error in measuring trafficking.

It might be argued that OSN data are unusually poor compared to other trace-based indicators, due to police inconsistency in requesting traces of guns with OSNs despite the stated commitment of YCGII cities to submit *all* such crime guns for tracing. Therefore, as a robustness check, we re-estimated the equations for criminal gun possession and violent crime rates using an alternative, though probably inferior, indicator of trafficking prevalence. Our correlation and principle component analysis results suggested that the percent of crime guns traced to dealers 250 or more miles from the city where they were recovered (DLR250ML) was the next-best trafficking indicator after OSN. When this was used as the proxy for trafficking prevalence, results were even less supportive of the hypotheses that trafficking affects criminal gun possession, or violent crime rates. The estimates for this alternate proxy are shown in the last row of Table 5. The coefficients are all negative, though nonsignificant. Thus, even if one believed that OSN data were more problematic than data for other indicators, the results still lead to the conclusion

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194.   *See* John M. Bruce & Clyde Wilcox, *Gun Control Laws in the States: Political and Apolitical Influences, in* THE CHANGING POLITICS OF GUN CONTROL 139, 150 (John M. Bruce & Clyde Wilcox eds., 1998).

that the prevalence of gun trafficking, measured using the two best proxies, is not significantly related to criminal gun possession or violent crime rates.

CONCLUSION

The model of criminal gun acquisition underlying lawsuits based on claims of negligent distribution is largely a myth, composed in part of rare and unrepresentative anecdotes about a handful of genuinely corrupt licensed gun dealers and misinterpreted ATF trace data. In contrast, the following conclusions are supported by the strongest prior research on the movement of guns to criminals, and the results of the empirical research reported in this paper:

1.   Time-to-recovery (TTR, or "time-to-crime") measures are not trafficking indicators. They more likely are indirect indicators of the gun theft rate, with which they are far more strongly correlated.

2.   High trace counts for FFLs are not indicators of trafficking by FFLs. They are, first, indirect measures of gun dealer sales volume and of local gun ownership levels. In places where there are more gun owners, there are more guns sold by licensed dealers, and eventually more guns stolen and found in the possession of criminals. Second, high trace counts are indirect measures of the rates of gun theft prevailing in the areas served by the FFLs. No research has ever shown high trace counts to be even weakly correlated with a dealer's identification as a trafficker once one holds constant the dealer's sales volume and gun theft rates prevailing in the areas served by the dealer.

3.   The only variable that is likely to be a strong city-level measure of gun trafficking activity is the prevalence of obliterated serial numbers (OSNs) among recovered crime guns.

4.   Illicit gun selling is almost all done at a very low volume. Typical trafficking operations uncovered by law enforcement authorities handle fewer than seven guns each, and ATF uncovers fewer than fifteen high-volume (greater than 250 guns) operations in the entire nation each year.

5.   High-volume trafficking, with or without the involvement of corrupt or negligent FFLs, probably supplies less than 1 percent of criminals' guns.

6.   Trafficking, if validly measured by OSN prevalence, has no measurable effect on levels of gun possession among criminals, as measured by the percent of homicides committed with guns, and has no effect on violent crime rates. One likely explanation would be that nearly all traffickers' potential criminal customers have other sources of guns (especially the pool of locally stolen guns) and are not dependent on traffickers.

7.   These specific conclusions logically lead to the broad policy conclusion that even the best-designed strategies aimed at reducing gun trafficking are

unlikely to have any measurable effect on gun possession among criminals or on violent crime rates. In particular, lawsuits intended to make the firearms industry rein in gun trafficking involving the knowing complicity or negligence of licensed dealers are unlikely to have such effects.

We can learn something about the potential of such strategies by considering evaluations of existing programs aimed at reducing trafficking. Perhaps the best known effort to reduce gun violence by going after traffickers was the Boston Gun Project, implemented in 1996–1999. The academic architects of the Project have conceded that criminal gun possession probably did not decline in Boston, and that much-touted short-term drops in gang homicide could not be attributed to the "law enforcement attack on illicit firearms traffickers," since criminal cases against traffickers were made only after the drops in gang homicide had already occurred.[195] They also conceded that they had no firm evidence that "supply-side enforcement strategies have any measurable impacts on gun violence," though they nevertheless argued that these efforts somehow "increased the 'effective price' for new handguns."[196]

Their basis for this last claim was that the share of Boston's crime guns that were new (recovered within three years of initial sale) declined during the Project's implementation from 1996 to 1999, a drop that they interpreted as a decline in the trafficking of new handguns. In fact, this decline paralleled a 50 percent decline in the city's burglary rate over the same period, a decline that began years before the Project started. As soon as the burglary decline ended in 1999,[197] the decline in the new gun share of Boston's crime guns also promptly stopped.[198] Thus, the decline in new handguns that the authors perceived as evidence of a decline in one type of gun trafficking was more likely due to a drop in the burglary rate, and thus the gun theft rate.

Similarly dubious interpretations of trends in short-TTR guns afflicts the efforts of Webster, Bulzacchelli, Zeoli, and Vernick to assess the impact of police stings directed at suspect FFLs in Chicago, Detroit, and Gary, Indiana in the late 1990s.[199] The authors concluded that the stings caused a decline in Chicago in corrupt FFLs channeling guns to criminals, based on the declining share of traced crime guns that were recovered from a criminal who was not the original possessor, and that had a short TTR (this share

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195.   *See* Braga & Pierce, *supra* note 10, at 722–23.
196.   *Id.* at 741.
197.   *See* FEDERAL BUREAU OF INVESTIGATION, U.S. DEP'T OF JUSTICE, UNIFORM CRIME REPORTS FOR THE UNITED STATES 1996, at 87 (1997) [hereinafter FBI 1996]; FBI 1997, *supra* note 105, at 90; FBI 1998, *supra* note 105, at 85; FBI 1999, *supra* note 105, at 85; FBI 2000, *supra* note 105, at 88.
198.   *See* Braga & Pierce, *supra* note 10, at 740 tbl.3.
199.   *See* Webster et al., *supra* note 26, at 229.

increased nonsignificantly in Gary).[200]   The authors failed to note, however, that over the period studied, 1996–2001, the burglary rate declined by 39 percent in Chicago and 62 percent in Detroit,[201] implying similarly huge drops in gun thefts, which would in turn result in fewer crime guns with a short TTR.   Thus, the patterns among traced crime guns that the authors observed could be entirely due to the decline in gun theft rather than stings of licensed dealers.

Theft is central to criminal gun acquisition.[202]   Interviews with incarcerated felons indicate that most guns acquired by criminals were probably stolen at some time in the past.[203]   Most gun theft is a by-product of residential burglary and other thefts from private owners.   Less than two percent of stolen guns are stolen from dealers and other licensees.   Only 12,302 gun thefts from FFLs were reported in 1997,[204] compared to about 618,000 total gun thefts, based on victim survey estimates.[205]   Unlike gun sales by traffickers, every gun theft by definition places a gun directly and immediately into criminal hands. Further, the known volume of gun theft is many times higher than any evidence-based estimate of the volume of trafficked guns.

One could speculate that even though virtually all known traffickers handle very small numbers of guns, there are many high-volume dealers who are too smart or lucky to be caught.   One might also speculate that even though trafficked guns known to authorities are few in number, traffickers actually sell large numbers of undiscovered guns.   One could also speculate that, unknown to criminal buyers, a large share of the guns they bought had been moved by professional traffickers further back in the chain of possession. There is, however, no affirmative evidence to support any of these speculations.   The view that organized or large-scale trafficking is important in arming American criminals is based not on strong evidence but rather on

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200.   *Id.*
201.   *See* FBI 1996, *supra* note 197, at 123, 128; FEDERAL BUREAU OF INVESTIGATION, U.S. DEP'T OF JUSTICE, UNIFORM CRIME REPORTS FOR THE UNITED STATES 2001, at 130, 137 (2002).
202.   *See* Cook et al., *supra* note 10, at 80–84.
203.   *See* WRIGHT & ROSSI, *supra* note 14, at 17 (reporting that 70 percent of felons surveyed reported their most recent handgun acquisition had either been directly stolen by them, definitely stolen by someone else, or probably stolen by someone else).
204.   U.S. BUREAU OF ALCOHOL, TOBACCO & FIREARMS, DEP'T OF THE TREASURY, ATF ANNUAL REPORT 1997, at 19 (1997).
205.   There were about 281,080 gun theft incidents in 1997, times 2.2 guns stolen per incident. *See* BUREAU OF JUSTICE STATISTICS, U.S. DEP'T OF JUSTICE, CRIMINAL VICTIMIZATION IN UNITED STATES, 1997 STATISTICAL TABLES, tbl.84, *available at* http://www.ojp.gov/bjs/pub/pdf/cvus97.pdf (last visited May 27, 2009); COOK & LUDWIG, *supra* note 34, at 30 (dividing number of guns stolen in noncommercial theft in 1994 by total number of gun-owning households that experienced the theft of at least one firearm that year).

(1) claims phrased in terms so vague and ill-defined as to render the assertions meaningless or trivial, (2) isolated anecdotes about unrepresentative, extremely rare large-scale trafficking operations uncovered by law enforcement authorities, and (3) dubious interpretations of highly ambiguous gun trace data. These are not sound bases for making public policy.

Virtually everyone believes that unicorns are mythical creatures. This belief is not, however, attributable to some scientific demonstration that unicorns do not exist. It is logically impossible to prove a negative, and previously unknown species are discovered all the time. Rather, unicorns are regarded as mythical because there is no reliable affirmative evidence that they do exist. Likewise, though a handful of large-scale gun trafficking enterprises are uncovered each year, there is at present no reliable evidence to affirmatively support the view that such traffickers are common enough to be important in supplying firearms to criminals, either in the nation as a whole or in any major local jurisdiction. Nor is there any reliable affirmative support for the theory that corrupt or negligent dealers play a significant role in supplying guns to traffickers. It is in this sense that the belief that big-time traffickers, or corrupt licensed gun dealers, significantly contribute to the arming of America's criminals is a myth. Indeed, there is no sound empirical foundation for the belief that any type of gun trafficker, as distinct from burglars and other thieves who occasionally sell guns they have stolen, has a substantial effect on the share of criminals who are armed with guns.